

Psychological Review

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THE PSYCHOLOGICAL REVIEW

EYE MOVEMENT AND VISUAL FIXATION DURING PROFOUND SLEEPINESS

BY W. R. MILES AND H. R. LASLETT¹

Stanford University

I

Before the great decrease in the activity of the postural musculature which comes with sleep (5) is observed, a person who is profoundly in need of sleep usually betrays his condition by characteristic behavior of the eyes and the tissues which surround them. The eyes may be said to show a specific sleep-activity. The movements become slowed, the lids tend to close, the eyeballs roll upward and glide about aimlessly, and the pupils contract. Obviously, some of these activities reduce or shut off stimuli to the retina. The sleepy person bats the eyes frequently, and may rub them quite vigorously, as if to remove irritation from the cornea. Sometimes it is necessary forcibly to lift the upper lids with the fingers, so marked is the sleep ptosis from what seems a temporary nuclear paralysis of the levator muscle.

It is fairly certain that sleep as an oncoming process or condition is not primarily a state of the external muscles, but more a condition of the retinas and visual centers. As a usual thing, the activity of the external muscles may seem to intervene, and therefore to precede the condition in the retinas. If the individual is desirous of sleeping, this is the natural sequence. But it has been shown by objective records

¹ Dr. Laslett through his research on the loss of sleep provided the opportunity for the taking of the records presented in this paper. The other author was responsible for photographing the eye movements, working up the records, and preparing this report.

(9) that the retinas or visual centers may go to sleep while the external muscles are still maintaining a suitable opening of the palpebral fissure, and also maintaining exploratory movements of the eye. Such a reversal in the usual order of events for the onset of sleep comes about at times of struggling against sleep.

The recti muscles, the orbicularis oculi, and the levator palpebrae superioris are reasonably subject to voluntary control, as one can easily demonstrate in absolute darkness. It is not clear that the sensitivity of the retina by itself can be equally well kept up or 'exercised.' In fact, the natural method of keeping the retinas active is through massaging the eyes by winking vigorously and rolling them widely. Every jerk of the eyes by the attached muscles performs a mechanical stimulation of the retinas at the areas corresponding to the points or bands of insertion of the muscles at the backs of the eyeballs. These stimulations are sufficiently strong to be easily seen as crescent-shaped phosphenes in the unadapted eyes, for example, immediately after entering a dark room. Forcible lid closure and also light rapid winking cause subjectively visible retraction of the eyeballs into their sockets (10) and also elevation of the corneas or lines of sight by 5 to 15 degrees (11). Such periodic distortions in shape, increases in pressure, and massagings by tightly fitting lids and closely squeezing muscle bands stimulate retinal and general ocular circulation and fight off the deadening effects of adaptation. This order of oculo-muscular events is another argument favoring the view that the retinas tend to go to sleep before the eyelids.

It seems most probable that sleep is of central origin, and that there is a more or less well-defined sleep center. This point of view is maintained by many observers of pathological material. Haenel (3) from an analysis of disturbances of sleep, and particularly from investigation of patients who exhibit encephalitis lethargica, indicates the probable placement of the sleep area in the mesencephalon. Hess (4) has demonstrated through a remarkable series of experiments on cats that it is possible to produce sleep with an astonishing

promptness and with the characteristic temporal course of all the symptoms, through the stimulation of a region localized along the axis of the brain and adjacent to the ventricles. Hess does not discover a narrowly confined sleep-center. The effective excitatory parts "lie in the same strata for which regulatory relations to the vegetative functions are assumed." He holds that the sleeping process is on a level with the vegetative reflex-functions. Well-regulated or normal sleep seems therefore to depend upon the anatomical integrity and physiological condition of this area in the central nervous system.

The eye as an indicator of sleep and sleepiness plays its leading rôle solely by virtue of the fact that it is the leading receptor mechanism, and because the body usually regulates its activity on the basis of visual stimuli. Furthermore vision happens to be the sense which has well-marked cut-off mechanisms associated with it. The tips of the fingers and the tip of the tongue have a high degree of cutaneous sensitivity, but with the onset of sleep they do not assume any particular posture nor are they covered up in a manner different from that of the waking condition. Similarly the olfactory, auditory, gustatory, and other senses are lacking in mechanical means for reducing sensitivity.² It is probably as easy to detect sleepiness in the voice as in the eyes, except for the fact that the organism usually makes a more continuous demand upon the eyes and thus gives rather better objective and subjective opportunities for observing the sleep tendency.

II

Sleepiness has not been a very fruitful subject for experimentation in human psychology. The difficulties are obvious. Sleep and sleepiness ordinarily mean the extinction of functional processes, but whenever one makes some particular feature of behavior the significant thing for observation, the

² It is possible that there may be a withdrawal mechanism in the case of the ear. If the tensor tympani proved to be more active during sleep, exerting a greater pull and causing the tympanic membrane to be less sensitive to all stimuli, we would have a condition somewhat parallel to that represented by the contraction of the pupil of the eye during sleep.

subject, recognizing this selection, can so organize his responses as to give an even performance in the line that has been selected. If promptness in reacting with the hand is the feature being measured, the ordinary individual can so reinforce himself at the moments just prior to and during activity as to appear practically normal. In between stimuli or trials he may profoundly relax and discontinue the coördinated processes.

Sleepiness is so astonishingly variable. It may be of profound intensity one moment, and then, within a very few seconds, due to a change of posture, the setting of the muscles, or the occurrence of some mental association, it may seem to be almost negligible. The organism that has been denied sleep for many hours will have these periodic attacks, but in between attacks of special sleepiness there will be experienced a continuing state of stress and subjective disturbance of the normal functioning of voluntary processes, provided the person is not strongly excited. It would appear possible to make measurements of this condition if properly chosen tasks were used.

Some time ago an intensive experiment in this field was conducted at Stanford by one of the authors (6). A group of five men went without sleep, so far as this may be done, for a period of 72 hours. A battery of measurements was employed at intervals during this sleep fast. The main results have been published, and it is not our object to review them here. When the investigation was in progress we used in our battery of tests, part of the time, a measurement which was the photographing of the saccadic eye movements and the fixations of two dots arranged horizontally, 40° apart. This eye movement test was of the identical sort devised by Dodge (1) and used by him and Miles (7) in alcohol and other experimentation. The subject, for a period of about six seconds, looked successively from one to the other of two marks, in rhythm with a metronome beating 80 per minute. The two fixation marks were definite and prominent (medium-sized black pinheads), on a curved light background 18 inches away from the eye. The pinheads were in a horizontal line with

the camera lens, and other pins at distances of five degrees were intermediate between the two *extreme* ones that served as the fixation points for the test. The left eye was covered by a comfortable blind, so that the fixation points were visible only to the right eye. The movements were recorded by the use of Dodge's falling-plate camera, on plates $2\frac{1}{2} \times 7$ inches. The fixation points were symmetrical in reference to the primary line of regard (20° on each side), and were in the most natural and convenient position for seeing. An arc light was used for forming the corneal reflection on the eye to be photographed, and a time wheel interrupting the beam of light gave one-hundredths of seconds, while the lenses were so placed as to provide a magnification of nine times. The apparatus was that shown in the article by Miles and Shen (8)—(see Figure 1)—without the copy holder. When one man was being photographed the others stood about near, so there was a certain amount of social stimulation tending to keep the men awake.

We made eye-movement photographs in only a few periods in the 72-hour experiment. The photographic plates were all from one lot, and when those for the earlier tests were developed, they proved to be absolutely spoiled. This was naturally a great disappointment, and we considered the effort a total loss. The plates used in the later tests during the 72-hour experiment were put aside and not developed, under the supposition that they would also be complete failures. Several months after Dr. Laslett had finished his work and left Stanford, the senior author discovered part of these old plates stored in the dark room. It was decided to take the chance of developing them, and to our surprise, some were found to have been sufficiently sensitive to record legible eye-movement photographs. It turned out that one or more plates for each of the five men, taken in the period from 2:10 to 2:50 A.M. on the last night of going without sleep, were reasonably legible. The results from these records are tabulated in Table I. Saccadic movement time is given separately for L (adductive) and R (abductive). The average size of the corrective movements in degrees is also given. From the inspection of this

TABLE I
AVERAGE EYE MOVEMENT VELOCITY AND CORRECTIVE MOVEMENT AMPLITUDE FOR
FIVE MALE COLLEGE STUDENTS DEPRIVED OF SLEEP FOR 66 HOURS

Subjects	Left Movt. Sec.	Corr. Movt. Degrees	Right Movt. Sec.	Corr. Movt. Degrees
I.....	.10	+2.2	.09	+3.0
II.....	.14	+1.6	.12	+0.5
III.....	.12	+1.0	.12	+2.0
IV.....	.14	+3.6	.13	+2.1
V.....	.12	+2.6	.11	+1.3
E*.....	.15	+2.0	.14	+1.5
Average.....	.124	+2.2	.114	+1.8

* These results are for the experimenter, Dr. Laslett, and have not been counted in the average.

table it is seen that abductive movement is more rapid. This is characteristic when recording from the right eye, as has been shown in earlier publications. The saccadic time for abductive movement for the five subjects ranged from .09 to .13, with an average of .114 σ . The range for adductive velocity was from .10 to .14 sec. Two of the subjects showed this longer time. The average for the left movement is .124 σ . Both of these averages are well above the normal speed for this test. A group of 63 college men, who were studied in the same manner in the evening, when they had been without sleep for approximately 14 hours, showed an average for L of .095 and for R of .089 σ . Only ten men of this group of 63 showed eye-movement speeds as slow as the average for our group of five men who had been without sleep for 66 hours. Dodge (2) gives a few results on himself (L .12 to .14 and R .11 to .13 sec.) representing his condition when he had supposedly been without sleep for 16 to 18 hours. These comparisons reveal the important fact that although the saccadic speed is somewhat slowed, due to loss of sleep, still these movements do not change sufficiently to interfere seriously with visual function. The lines of time dots indicate that the sleepy eye still retains its ability to accelerate rapidly at the beginning of each movement. The great change, the conversion of the saccadic movements to the

rolling type, occurs only at the moment of sleep onset, as shown in a former paper (9).

Every one of the subjects demonstrated corrective movements; that is, the large saccadic shifts were not sufficiently accurate to bring the gaze directly from one prearranged fixation point to the other. It is normal to show these corrective movements, although they may be absent or much reduced after the same fixation points have been practiced on. The eye typically does not overshoot, but stops before reaching the mark, and then adjusts by a smaller corrective saccadic movement. The peripheral view, 40° away from the fovea, on the basis of which the first long movement is elaborated, apparently cannot produce just the amplitude of movement necessary. This normal feature of eye-movement records did not disappear for the men who had gone without sleep, but rather appears to have been intensified in that the correctives are of larger amplitude than normal for this test, and are more often double. The usual corrective is about 1° , whereas with these sleepy men it averaged 2° and was sometimes as much as 5° to 8° . This change seems probably due to a decrease in the sensitivity of the retina and its response to peripheral stimulation. When the eye-movements require twice as much time as normal, as they are apt to just prior to the moment of sleep onset (9), correctives may entirely disappear, as it seems possible for the person to fixate on the basis of the cues received during the very slow termination or deceleration of the saccadic sweep. This is quite similar to changes in fixation through accommodation where during convergence or divergence the movements of the double images can be noted. Doubtless these serve as useful cues in achieving successive accommodation fixations.

Use of the metronome prompted to rhythmical movements and fixations, and thus tended to stimulate the subject to some extent. The instructions were to fixate at every click of the metronome. Eighty shifts per minute is not at all difficult for the ordinary subject. It will be recalled that in reading the eye usually pauses one-fourth to one-third of a second, and the shift in the case of our eye-movement test

should not require more than one-seventh second at the very outside. Thus it would be easily possible to provide for eighty movements and eighty fixations within a minute. Our five sleepy subjects were always able to keep up with the metronome, but their fixations differed considerably from normal visual fixations. It is typical for the rested wide-awake subject to hold the eye quite still (but not static) during a period corresponding to about a half a second. This inscribes a vertical line of fair width and sharpness on the plate. Inspection of the records under consideration reveals the fact that concise vertical lines are the exception. About three-fourths of all the fixation records in this legible group of plates show wavering or drifting of the eye during the fixation period. Wavering does not seem to be made up of small saccadic jerks, but is rather a slight rolling movement to one side or the other. In a good many instances the 'fixation' has the characteristic of a slow, diagonal drift, often toward the next fixation point. One might imagine that in such cases the retinal cues, ordinarily perpetuating the fixation, are in these instances less clear or less effective, and that the rhythmical movement habit projects itself forward to the next position.

There are a good many eye closures or winks shown in the records, but not as many as might be expected from such subjects. This is due, doubtless, to the fact that always in instructing the subject just prior to recording, he was told, "Now keep the eyes open and follow the metronome." This usually resulted in two or three winks by way of preparation, and these served to see him over the six seconds of the actual test.

III

The qualitative characteristics of the fixations and of the eye-movements which preceded them can best be discussed in connection with some illustrative records. In Plates I and II a total of nine records are reproduced. These are for Subjects I, II, IV and V. In Plate II we reproduce records 55 and 56, which were taken on Dr. Laslett immediately following the records made on Subject V at 2:50 A.M. Dr. Laslett was

responsible for keeping the subjects awake, and for putting them through practically the entire battery of measurements at the different intervals after the sleep fast had begun. He was under much more strain than any one of his subjects. He kept constantly watching his five men, providing various diversions for them, keeping them from nodding in short naps, and from periods of nearly complete relaxation when they might be sitting or standing leaning against the wall. He had some assistance from other people, to allow him some short periods of sleep, but actually, in connection with the whole experiment he had robbed himself of almost as many hours of sleep as he had taken from his subjects. It is justifiable to include his records among the illustrations, although they have not been included in the averages in Table I. His records show as much, and in some particulars, more disturbance from the normal than is found for the sleepless subjects.

A survey of all the nine reproduced records shows that the saccadic movements (nearly horizontal lines of dots, each equal to .01 sec.) are in general fairly straight and free from curve. This is an indication of rather quick lateral eye-movement without much upward roll of the eye which is the usual accompaniment of winking (II), and in some degree is normal for wide lateral excursions of the eye. Curved or indirect saccadics show in the upper portions of some records; for examples see 39 and 51. The general indication, however, is for reasonably prompt, direct shifting of the line of sight from one fixation point to the other.

A saccadic eye-movement curiosity not before met with occurs in records 49 and 50. Here the subject, IV, for a part of the time in the upper or latter half of both records, broke the long saccadic movement into short movements. Midway of the 40° angle the eye paused for a period approximately as long as the clearing up period which usually preceded the corrective movement at fixation. In none of these pauses is there any indication of a corrective movement. There was no particular point that the individual was trying to see or fixate in this region; the eye simply stopped for a

moment, and the stop was at different places at different times. Such a record might occur if a subject made his own interpretation of the demand upon him in reference to the experiment. This person knew perfectly well what to do and had been doing it. He lapses into a pattern that was apparently more simple for him in his sleepy condition. There is a further curiosity in the small pause at the very beginning of the last abductive movement in record 51. This little pause at the end of fixation is too short for a clearing-up period. A similar brief interruption occurs in both record 2 and 4 of Plate II in the former paper (9). Such features have not been noted in the records of subjects normally awake.

Corrective movements for adjustment of fixation are larger at the first part of each record, and this is normal. The first shift of fixation from right to left occasioned a large corrective movement in seven of the nine illustrated records, and most of these corrections are considerably larger than normal. In two cases, records 50 and 55, the correction was double; that is, two corrective movements in the same direction followed each other. This is an exceedingly rare occurrence in eye-movement records. The two first left-hand fixations in record 55 demonstrate very markedly double correction in the same direction. It is furthermore noteworthy that the two right-hand fixations following these also have double correctives, but they are in opposite directions. Record 37 contains very curious double corrective movements on the right-hand side. The subject corrected promptly upon arrival in the vicinity of the fixation point, but did it in a careless manner, as if giving very fragmentary notice to the retinal impressions; he went too far and as promptly moved back. This he did in three consecutive fixations on the right. The probable interpretation of this increase in size of correction, and frequency of double correction, is that the visual centers, being less active than normal, do less well in elaborating movements from peripheral cues.

In the records illustrated, several of the fixations after the corrective movement show drifting of the eye. This is particularly prominent in the upper parts of records, especially

in 42 and 56, and in the second right-hand fixation of record 50. Several of the clearing up periods (prior to corrective movement) also demonstrate this drifting character. Such drifting we assume may be due to lapse of attention, reduced sensitivity or, less likely, to reduced responsiveness of the eye muscles to retinal control. Probably all of these factors enter at times to produce the results that we have found.

Wavering fixation is typical of these records for the sleepy men. Of course the rate of the metronome permitted relatively long fixation, and it may be argued that returning several times to the same fixation point which does not move and has not otherwise changed is rather a tedious business, and would naturally cause the wavering of the fixation. To such a criticism it must be replied that individuals not in need of sleep maintain much steadier fixation under the same experimental conditions.

All of the records illustrated show the same number of cycles of fixation, but the duration of fixation changes a good deal within the same record, and among the different records. Most of the subjects show the longer fixations on the right. The mark at the right constituted the starting point at the beginning of each test.³ Probably in a sense it is accepted as home base. Another factor is that it seems to be easier for the eye to take up a position to the right than to the left. Movement of the right eye to the right is executed at higher velocity than the corresponding movement in the opposite direction. This may be a feature of the right-handed habit. The longer fixation pause at the right seems more characteristic of the sleepy subjects than of men who are wide awake.

The very end of record 56 demonstrates the phenomenon that was described in the previous paper, that is, the conversion of the typical saccadic movement preceding fixation into a rolling movement characteristic of the sleeping eye, which is not responsive to retinal cues. In his final approach

³ The beginning of record 37 was cut off through closure of the eye. Probably this record, and also the very first movement in records 52 and 56 are not entirely free from head movement.

of the left-hand fixation mark this subject made no clear-cut corrective movement; and he exhibits no vertical section representing a clear stable fixation. The eye starts back to the right hand fixation point with complex glide movement. This interval in the vicinity of the left-hand fixation point is one that we could properly characterize as the onset of sleep for the visual centers.

SUMMARY

1. Five young college men who had gone without sleep for 66 hours were studied by photographing their eye-movements and consecutive fixations of two points separated by a visual angle of 40° .

2. The speed of saccadic eye-movement under these conditions was about 30 percent slower than the average for comparable subjects on the same eye test, but was not so slow as to interfere seriously with vision.

3. The subjective condition of sleepiness modified the visual fixations more profoundly than it did the eye-movement velocity.

4. Corrective movements for fixation were larger and less exact in the sleepy individual than is normal, and double corrective movements even in the same direction were sometimes found to be present.

5. Wavering of fixation and slow drifting of the eye, to the right or left, during supposed fixation, was characteristic behavior for the sleepy men. Since eye-movements themselves remain fairly adequate, it is assumed that the difficulty with fixation is due to retinal or central changes.

6. When the visual effort to fixate ceases, the eye takes up a rolling type of motion, strikingly different from the usual saccadic adjustment.

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PLATE I.

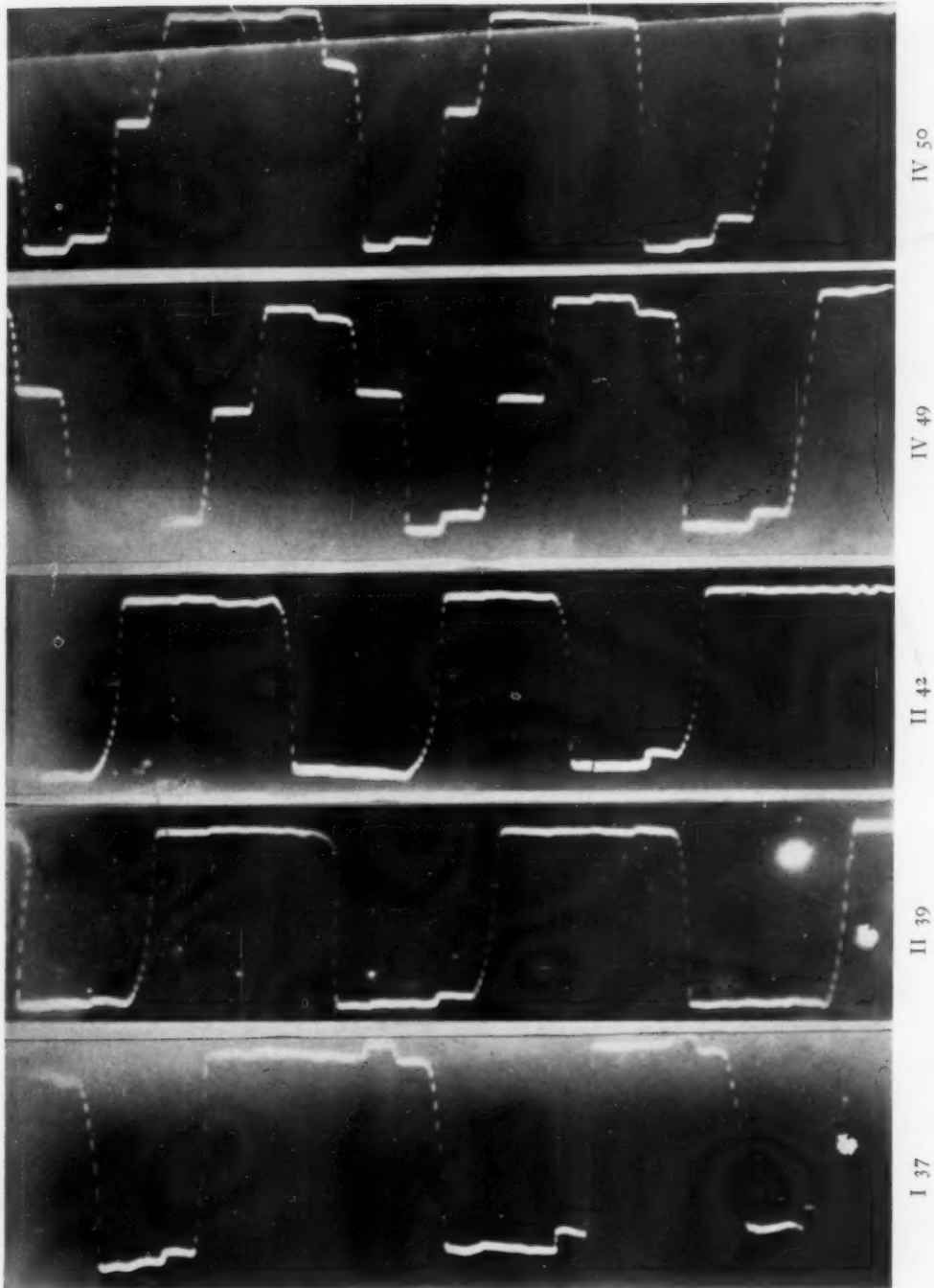


PLATE I. Illustrative eye movement and visual fixation records for men who have been without sleep for 66 hours. Note the wavering fixations and the interrupted movements.

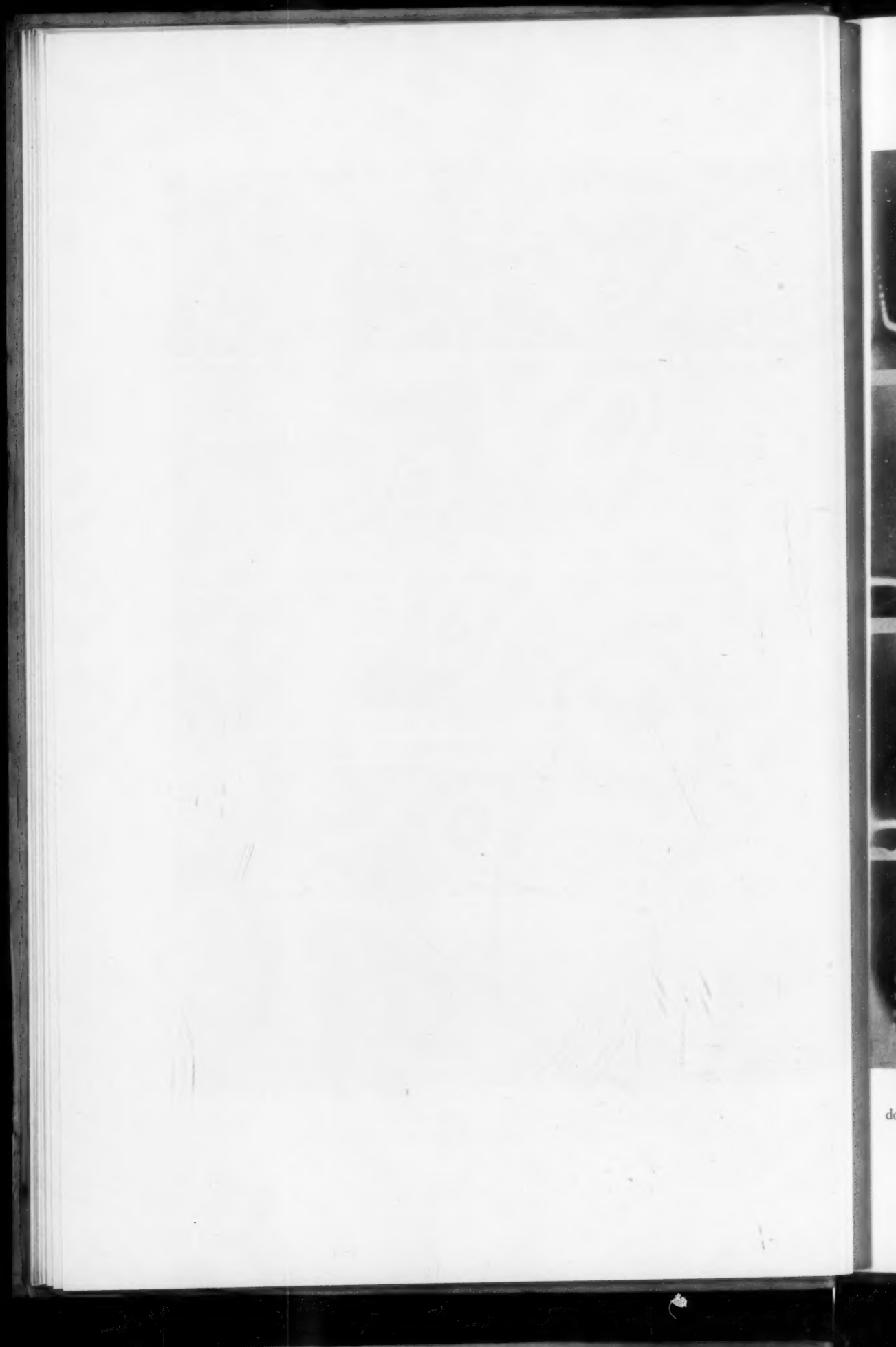


PLATE II.

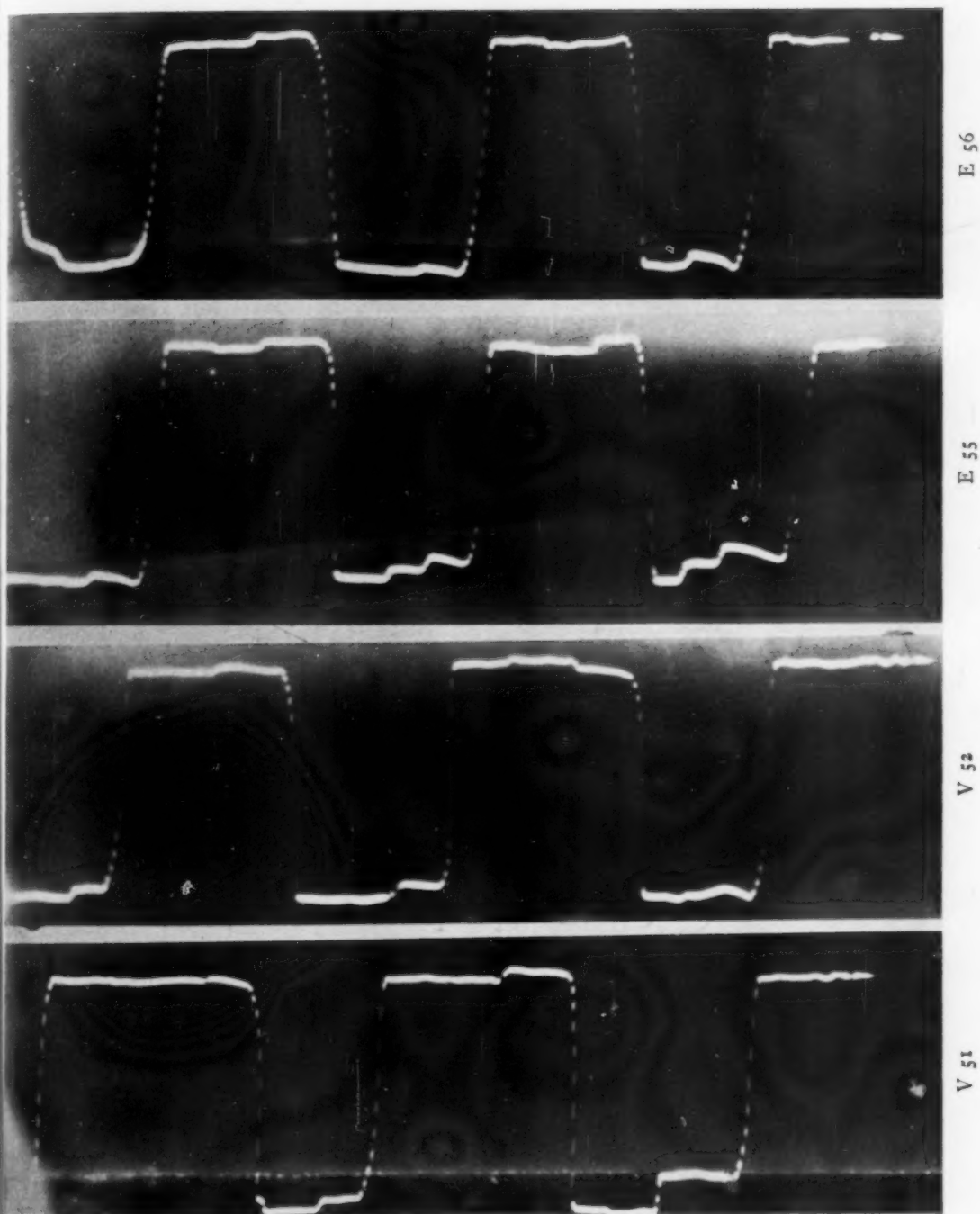


PLATE II. Sample eye movement and fixation records showing the effects of the loss of sleep. Note double corrective movements and the one complete failure of fixation at the end of 56.

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A PARADOX IN PSYCHOLOGICAL THEORIZING

BY THEODORE ADOLPH BRETSCHER

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Through the analytical method, the psychologist has succeeded in accumulating a wealth of factual material, but finds that he really lacks a workable scientific theory to explain these facts. Both mechanism and vitalism, the two old classical metaphysical systems, have been found wanting in serving as workable and comprehensive concepts. Interest is now focused on whether the 'organismic' or 'Gestalt' theory has anything to offer to remedy this condition.

The concept that reality with all its qualitative differences could be reduced to pure impact of mass particles, gave the modern man his most potent instrument in subduing and understanding nature, whereas under the old Aristotelian and medieval faculty physics, with its teleological explanations, it was impossible to compute causal laws with mathematical precision and exactitude. "If I can make a mechanical model," said Lord Kelvin, "I can understand it. As long as I cannot make a mechanical model all the way through, I cannot understand it." With this statement, Lord Kelvin sounded the keynote of Newtonian physics.

But paradoxical as it may seem, the farther man penetrates with this powerful instrument into the secrets of nature, the farther he finds himself from understanding real nature. For though he has at last found a workable theory which makes a disinterested, methodological procedure possible in the realm of physics and mechanics, he finds that this method, so productive in the field of physics, creates a seemingly unbridgeable chasm between so-called nature on the one hand, and life and mind on the other.

Deeply impressed by the methodological procedure which the mechanical summation theory made possible in the field of physics and chemistry, the psychologists in the later

nineteenth century attempted to construct an objective classification of mental phenomena. Both the Wundtian structuralists, and the molar and molecular behaviorists, though diametrical opposites in their views, attempted to analyze mental phenomena into constituent parts. Imitating the method which the mechanical summation theory placed in the hands of the physicist, the old structural introspectionists proceeded to analyze all qualitative experience into sensory units. Mental phenomena for these psychologists could be described and classified in terms of sensory components. Conscious life was deemed to be nothing but an integration or a summation of sensory elements which, through some semi-mechanical form of association, became organized into higher and higher complex units.

However, qualitative analyses do not lend themselves to the exactitude of measurements which science demands. It was chiefly this limitation which led to behaviorism. Watsonian behaviorism is simply the attempt to observe and measure the purely overt objective behavior. Its dictum, as we all know, is, "Given the responses, the stimuli can be predicted; given the stimuli, the responses can be predicted."

But strange as it may seem, both qualitative structuralism and quantitative structuralism, or, if you will, the Watsonian type of behaviorism, are dominated by intellectual positivism. They both seek, through the analytical method, to reduce mental phenomena to a scientific morphology of classification in order to eventually find certain computable structural laws. In short, their methods employ measurements and classifications of mental data.

Now, somehow or other, structural analysis, whether quantitative or qualitative, even though its value as a method of description and classification cannot be denied, tends to distort the living qualitative experiences of mental life, which do not lend themselves to any artificial reductions to elementary parts. For the very core and nucleus of conscious life, yes, the very sanctuary of our life, as William James puts it, is the sense of activity which certain inner states possess, and so consciousness, or all qualitative experience,

functions as a process or whole and cannot be analyzed into constituent parts.

It is functional analysis that leads ultimately to the questions, "What are the essential conditions for the existence of mental life?", "How is all this factual material to be recognized?", "What accounts for its existence?", or, "Why does it occur?" In short, the interest gradually shifts from observation and classification to an attempt to explain.

The limitations of the summation method as applied by both structuralism and Watsonian behaviorism in the field of psychology had three rather interesting and curious results.

The reader is asked here and in what will follow to distinguish between a method or methodological procedure on the one hand, and an explanatory system or theory on the other. Structuralism, as such, is primarily interested in method. For Titchener, for example, structuralism is wholly and solely interested in the analysis and classification of pure sensory experiences. In working with such categories as reaction time, intensity, and extensity, the Titchenerian structuralist is interested in description or, to use the German term, 'Beschreibung' and not, as such, in the question of cause, meaning or 'Kundgabe.' It is only the observed qualitative content of experience which forms the subject matter for him. The subject used for experiment must, as Titchener points out, avoid all so-called stimulus-errors. He, the subject, must only report the observed content of experience and not the stimulant as such or the inner meaning as such. In short, the subject must report and describe an experience such as anger or fear in a purely disinterested fashion.

Realizing the handicaps of structuralism but at the same time unwilling to cast a method overboard which has been so productive in the field of natural science, modern behaviorism, especially that of the molar and molecular type, has tried to reduce all qualitative experience to quantitative terms. Consciousness and all qualitative experience are nothing but excess baggage which only handicap a really scientific and objective procedure. Hence, they must go overboard. Psy-

chology, as a science, can only work with categories which permit absolute measurements and quantification. It can only concern itself with observable public facts which can be analyzed and described in an absolutely scientific, disinterested fashion. "A psychology," as Watson puts it, "of interest to all scientific men would take as its starting point, first, the observable fact that organisms, man and animal alike, do *Adjust*¹ themselves to their environment by means of hereditary and habit equipment. These *Adjustments* may be very adequate or they may be so inadequate that the organism barely maintains its existence; secondly, certain *Stimuli* lead the organism to make the *Responses*. In a system of psychology completely worked out, given the responses the stimuli can be predicted; given the stimuli the responses can be predicted."²

Let us look at the last sentence of this quotation. A fine program, a perfect application of the analytical method in an absolutely objective field!

Now our old friends the structuralists have been handicapped by trying to apply the analytical methods to a subject matter where it is inapplicable, while our good friend the Watsonian behaviorist finds himself with the analytic method on his hands, but a very thin and meager subject matter to which to apply it. Due to this poverty of real subject matter his interest soon begins to shift from that of pure description to explanation or, in short, from objective classification and description to metaphysical speculation. Since he has discarded, or tried to discard, from his vocabulary all vitalistic and mentalistic categories as being too subjective, he has only one recourse, or rather, he sees only one outlet, and that is the application of the Newtonian mechanistic concepts in the field of psychology. In short, the ultimate result is that psychology and biology are nothing but parts of physics and chemistry.

But let us take another glance at the quotation from Watson. The Watsonian program calls for elimination of all

¹ *f. italics are the author's.*

² J. B. Watson, *Behavior*, pp. 10-11.

terms which have any subjective or teleological coloring. But observe, in his definition, he uses terms like adjust and adjustment.

What meaning have such terms as 'adjustment' and 'adjust' in a mechanistic universe of discourse? Granting the probability or the possibility that our old crude mechanical concepts may be refined to take in, as Herrick points out, higher and more complex structures of reality; nevertheless, no matter how we refine our mechanistic categories, the terms adjust and adjustment, at least with their present connotations, have no intrinsic meaning in a mechanistic universe.

The only subject matter of interest for an outright behaviorist of the Watson type is mere mechanical activity or external movements; the external observable responses to the external observable stimuli. "Given the responses the stimuli can be predicted; given the stimuli the responses can be predicted." If we let *S* stand for stimulus and *R* for responses, we may say that, according to the Watson type of behaviorism, *S* — *R* forms a reflex unit; there is an absolutely mechanical one-to-one relation between *S* and *R*. The nearest approach to such merely mechanical movements found in human organisms are probably those of the patellar and pupillary reflexes, for example, those caused by physical stimuli such as contact, light and sound. Even here, as Woodworth points out, such a thing as an *S* — *R* unit in the full sense does not exist. Reflexes may be elicited by any one of a number of different stimuli.

Both the structuralist and the Watsonian behaviorist find themselves with a method on their hands which is inapplicable to their subject matter. For, as we have already pointed out, mental life cannot be analyzed into constituent parts without grossly distorting the picture. For conscious life or qualitative experiences act as a process or as an organic whole, rather than a passive or static 'tabula rasa,' on which impressions are made. The organism rather molds its environment more than the environment molds the organism.

The limitation of the analytical method in reaching this deeper functional aspect has led some of our modern psychol-

ogists to again emphasize a sort of teleological or voluntaristic factor in mental life. Psychology, instead of being concerned with purely static elements, is now, for these functional psychologists, primarily interested in living things. This shift of interest from the formal static conception of nature back to the old Greek concepts of living organism is, of course, primarily due to the romantic reaction against classicism in the nineteenth century and to the revolution in biology since the time of Charles Darwin. Such old Aristotelian categories as 'purpose, striving, ends,' became the tools again for the modern functional psychology. Psychology, as McDougall defines it, is a positive science of the behavior of living things.

But in accepting such vitalistic and mentalistic principles as causal explanation of subject matter, the functional psychologist finds himself with a category on his hands that violates the sacred canon of natural science: "Thou shalt not permit any anthropomorphic principle to enter into thy scientific concept."

Thus, while the structuralist and the behaviorist have tried to work with a method which is in conformity with the canons of modern science, the functionalist tries to work with a theory or system which is anthropomorphic and a violation of all canons of natural science. While the structuralist and the behaviorist try to work with a method which, though it has all the indorsements of science, distorts the real picture of mental life, the functionalist tries to work with a theory which, though it gives a seemingly truer and more real picture, does not lend itself to scientific use.

Our discussion so far has primarily concerned itself with two antithetical modes of philosophical thought, which are nothing but an expression of certain old historical ways of thinking.

Mechanism considered as a workable theory in scientific thought is dominated by philosophical, intellectualistic positivism. It is analytical and deals only with summation of constituent parts.

Bergson, the arch-enemy of all forms of intellectual positivism, may be right in his criticism of analysis when he calls

it nothing but an artificial geometrizing of nature by the intellect. Nevertheless, artificial or not artificial, man, up to the present day, has not been able to devise any objective workable method other than that of analysis. An 'Élan vital,' 'Entelechy,' or any other such vitalistic and mentalistic category, though it may be valuable in giving us somewhat of a picture of the real inner core of mental processes, is not applicable to what is called an objective, disinterested form of scientific procedure.

Bearing in mind these limitations of mechanism and functionalism, we are now ready to discuss briefly the Gestalt theory, which is rapidly gaining ground amongst American psychologists.

The development of the Gestaltqualität concept, which originated among the German psychologists since 1900, is simply a phase or an aspect of anti-intellectualism in the field of philosophy and a drifting away from the old absolutistic point of view, which dominated physics for several centuries, to a point of view from which the laws of nature are conceived to be relative and not fixed. Influenced by these general tendencies in the field of philosophy and natural science, psychology in the present day is endeavoring to work out a theory which tries to do full justice to the qualitative and synthetic aspect of mental life without in any way calling in the extraneous agents of vitalism and mentalism.

The starting point of the whole 'Gestalt' movement is its antagonism to both teleological and mechanistic principles.

In the first place, it rejects the analytical method as well as the analytical attitude toward a problem. Mental life is not a product of either physical or sensory integrations. The whole cannot be explained by nor reduced to the summation of its constituent parts. Qualitative experiences are always and at all times a homogeneous continuum. To quote Köhler: "I look up to the homogeneous blue sky of today and find it continuous. Not the slightest indication of its being composed of real units, nothing of limits or of any discontinuities. One may answer that my simple observation is not the method to decide this point, but I cannot agree with this

argument, since we need, first of all, concepts for the understanding of our immediate experience; and the sensation loses a considerable share of its importance as a fundamental concept, if, taking it as something of the molecule type, we find nothing to substantiate this idea in direct observation. The continuity of that region of the sky or of any homogeneous field is a positive property of it. And we see that our fundamental theoretical concept in this form does nothing to make this property understood. On the contrary, a special hypothesis would be needed in order to explain how, in spite of the existence of sensation molecules, the homogeneous field becomes a continuum. Therefore, the only thing produced by this useless assumption is a complication of theory. And I say the more stress on this fact, as we shall see very soon, that there do occur parts in sensory fields which are real objective units, though they certainly are not 'sensations.' The concept of sensation tends to hide for us the importance of these other realities and has done so for a considerable time. . . ."³

Another characteristic worth mentioning for our purpose, is the fact that the wholeness concept is strictly naturalistic. In the interpretation of the subject matter, the Gestalt psychologist claims to deal with strictly naturalistic and objective categories. These 'Gestalten' or configurations, whether physical or phenomenological, are for Wertheimer, Köhler and Koffka cosmic in structure.

Being cosmic in structure, the Gestalt psychologist does not have to assume any vitalistic or mentalistic synthesizing agents. Under the old empiricistic interpretations of optical perception, an object, such as a book or an orange, was nothing but an integration of physiological or mental elements. Under the functionalistic or nativistic interpretation, the integration of these elements was a result of an inner activity. Both of these traditional interpretations the configurationist has discarded. Neither the form nor the color of an object is due to either a summation process or to mental or vital agents. In the field of optical perception, from which the Gestalt psychologist has gathered nearly all his material,

³ W. Köhler, 'An aspect of Gestalt psychology,' in *Psychologies* of 1925.

such qualities as figure, form, color, are neither physical nor mental in the old sense of the term. They are, as they tell us, cosmic or natural forms.

Cosmic structures or configurations, neither physical nor mental in their essence! Indeed very strange revelations to one who is brought up under the old Newtonian concept of causality. What then is the nature or the cause of these configurations? If these configurations, which the new qualitative psychologists talked about, are not to be explained by a mechanistic or vitalistic theory of causality, what then is their explanation, or how does mental life come into existence? And this leads us to the third and last characteristic to be examined in this discussion.

For mechanism, mental life and qualitative experience were ultimately reducible to some form of mechanical energy. For the vitalist as well as the mentalist, mental life can only be explained by assuming some extraneous agent such as an 'Elan vital,' 'Entelechy,' or a transcendental Ego. For the Gestaltist, mental life or qualitative experiences are neither reducible to a primitive form of energy, nor are they due to some extraneous synthesizing agents. But they are causally explained by a form which is called 'emergence.' But what do we mean by emergence or emergent evolution?

Under the old traditional conception of causality the whole is either causally determined by and reducible to purely quantitative entities or stuff, or it is caused and determined by separate extraneous agents. The first leads to a materialistic monism, while the second leads to Cartesian dualism or to pluralism. In the first alternative the qualitative wholeness of experience is causally determined by the sum of its aggregate parts; in the second alternative the wholeness is causally determined by an extraneous synthetic agent.

For the emergent evolutionist the wholeness of qualitative experience is neither reducible to the sum of its aggregate parts, nor is it due to any vitalistic or mentalistic synthesizing agent.

Reality, in accordance with Lloyd Morgan, forms a hierarchy of increasing complexity of configuration. Each new

configuration cannot be reduced to the sum of its individual primitive parts. Thus, chemical phenomena are emergents of physical configurations. Life is the emergent of the physical-chemical configurations, and so on up. Each new configuration has its own laws, which are not reducible to its substructive components. Mind, then, though based on the primitive substructure, cannot be reduced to the laws which are valid in the lower substructures. Or, to put it in abstract terms, the emergent theory asserts, as far as I can understand it, that the whole which may be composed of, let us say, constituents *A*, *B*, *C*, takes on a new form or product in a certain relation (*R*), which the characteristic properties of the constituents *A*, *B*, and *C* do not possess separately. Neither can the whole, even in theory, be deduced from the most complete knowledge of the properties *A*, *B*, and *C* in isolation. An emergent quality, then, is not reducible to nor deducible from the sum of its individual components. The whole, though it may be determined by a primitive substructure, cannot be analyzed into any primitive stuff or structure.

Genetically, the wholeness or total quality is prior to the 'part properties.' The wholeness-characteristics in mental life cannot be described or explained by any combination of elements nor by assuming an extraneous synthesizing agent. Qualitative experiences are emergent facts which defy analysis. Analysis, the very foundation on which the whole edifice of natural and physical science has rested, is here to be discarded in favor of a causal concept which has been foreign to scientific thought.

That the emergent theory comes closer to our naïve realistic perception of life than the old summation theory of causal sequences no one will deny. From a purely factual point of view we cannot deny that the 'Gestaltqualität' theory may some day throw more light on neurological and psychological phenomena than the old summation theory has done in the past. We cannot close our eyes to the factual material which the research work of such men as Köhler, Lashley, and many others in the field of psychology and neurology, has brought to the surface. Neither can we cast overboard the concept of

emergent causality because it happens not to fit into our old traditional logic.

Granting all this, we cannot ignore the fact that the causal categories which the emergent evolutionists offer are nothing but a description of certain factual phenomena and no real causal explanation in the scientific sense of the term. Of course, when we talk about causal explanation in the finalistic or metaphysical sense, this same limitation can also be applied to the mechanistic theory. For the causal categories which mechanism offers are in the last analysis nothing but a description, if we want to squeeze that term. But we are not interested in any finalistic or metaphysical question, but in a scientific theory. Human understanding seems to be so constructed that causal laws or causal explanations, in a scientific sense, must somehow or other be conceived to be analyzable into some kind of components in order to be workable and intelligible as a scientific hypothesis.

Mechanism offers us a scientific explanation of causal sequences which is at least somewhat intelligible in the field of physics, though it may be wholly inadequate, as far as our present knowledge now goes, in the field of psychology. But the emergent evolutionist asks us to accept as scientific explanations causal laws and categories which are at the present entirely foreign to our old modes of comprehension and understanding. We may cherish the hope with Bergson and others that posterity may some day be delivered from the old thrall-dom of intellectualization and geometrization to some new mode of understanding and comprehension of causal laws. As yet, the old saying of Lord Kelvin holds true: "If I can make a mechanical model, I can understand it. As long as I cannot make a mechanical model all the way through, I cannot understand it." This intellectualization may utterly falsify and distort reality. Nevertheless, it is the only scientific method which we poor mortals possess.

CONCLUSION

Thus, the mechanistic view, which yields itself to an applicable and intelligible scientific method, creates a mental

being which is most unreal and foreign to all our experiences. Yes, we may say, it is the most irrational absurdity that has ever been spun out by the human imagination. On the other hand, the organismic or configurational point of view creates a being which comes within the confines of our experience. It shares with vitalism and mentalism the warmth and familiarity which at once will arouse the applause of common sense. It is, in short, close to what we mean by the term 'actual experience,' but this very closeness makes it again unintelligible and inapplicable as a methodological procedure.

For, squirm as we will, in calling the old analytical and rationalistic method a mere convention which has no real basis in the universe, nevertheless, convention or no convention, if scientific knowledge means a disinterested and realistic understanding of the phenomena of nature, it can only be got, at least as far as we know at the present day, by an analysis and classification of data.

But, the reader will say, we have seen that we never are able to penetrate the inner processes of mental life by any method of analysis, for these inner mental processes function as organic wholes and not as composites of elements, they are therefore beyond the reach of analysis. Is the implication then that the organismic or configurational point of view is of no use as a scientific theory? Yes and no. Yes, in the sense that it cannot be used as a workable hypothesis in yielding to measurements and computation. No, in the sense that it may sharpen man's higher critical insight of mental life and human relationship, thereby enabling him to have a better and more intelligible organization of mental phenomena. But, if we take it as a workable scientific hypothesis, then we must say that its vocabulary or terminology only adds more to our stock of meaningless terms. For what else are such terms as 'Emergent,' 'Gestalten,' 'wholes,' etc., but a blanket to cover our ignorance and to overawe the neophyte?

True, when we look at the mechanistic theory, with all its auxiliary concepts, in the light of its logical and metaphysical development, we shall find that it has traveled a

circuitous path. For none of its categories are founded on smooth logical thought. Even a superficial examination of the axioms and definitions on which modern mathematics and natural sciences are grounded will show that they not only violate the laws of thought but are in violent contradiction to all empirical experience. Now a theory may be in violent contradiction to logic and experience and still be workable. Paradoxical as it may be, the very fact that we are cognizant of these contradictions shows that they are not mere meaningless and empty terms, but, somehow or other, the real and only food on which the thinking organism seems to develop. On the other hand, such a term as 'emergent' may tickle our mental palate but it contains as yet no nutrition for the methodically thinking organism.

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A CONSIDERATION OF HUNTER'S CRITICISM OF LASHLEY

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Students of neural processes have long since seen the inadequacy of any static concept based on the accretion of discrete elements or parts, in explaining the behavior of the intact organism. In an attempt to postulate a theory more in accordance with the known facts of behavior Lashley, after extensive study of the neural mechanisms underlying behavior, presents his concept of equipotentiality and mass action. Hunter¹ has recently criticized this theory of equipotentiality, asserting that none of Lashley's facts are in discord with the current neural theory. The present paper will attempt to point out that Hunter's criticisms of Lashley are irrelevant to the issue involved, indeed are a misconstruction of the facts in question, and that Hunter's neural theory is non-committal with respect to any basic problem.

HUNTER'S NONCOMMITTAL STATEMENT OF CURRENT NEURAL THEORY

The first point to be raised in critique of Hunter is his statement of the current neural theory. He gives the following as a brief presentation of it and on this basis attempts to show that current neural theory explains the facts of animal behavior better than does Lashley's concept of equipotentiality. Hunter's statement reads: ". . . students of neural processes have gradually built up the theory that behavior is controlled by stimuli arousing neural impulses which pass over reflex arcs and so excite muscular and glandular responses. The central nervous system is not regarded as a mere conductor of such impulses, but also as an integrater of them.

¹ Walter S. Hunter, A consideration of Lashley's theory of the equipotentiality of cerebral action, *J. Gen. Psychol.*, 1930, 3, No. 4, 455-468.

To this conducting and integrating function current theory also adds the probability of some general neural capacity or plasticity (call it what you will) which sets the limits of accomplishment by the individual."² From the above statement it is difficult to see just what position Hunter is defending. He has said that this is the current neural theory, yet we find in his presentation only an implied set of assumptions relevant to the conventional reflex concept or to any neural theory. From passages later in Hunter's argument it is evident that he means by current neural theory the reflex concept, but he has so obscured this theory in his criticism of Lashley that his position is anything but clear with respect to the problem under discussion.

In order to clarify the issue between Hunter and Lashley it might be well to give some of the essentials of the current neural theory, which Hunter implies by his use of the reflex concept. They are as follows: (1) That the reflex arc is the basis of all behavior. It is the unit out of which all behavior is built; thus it is genetically and logically primary; (2) By definition these reflexes are entities, having their own properties in their own right; (3) That they summate; this is implied in the first point; (4) That the functioning of each is conditioned upon the activity of a definite set of neurones. In other words, there is a strict correlation between structure and function; (5) That one can predict from structure to function; (6) That somehow the brain is an integrating mechanism for the reflexes and facilitates and inhibits them; (7) That at least by implication there are, as well as spinal reflexes, central ones (cortical); (8) That reflexes do not blend (opposite in a way to point three) for if they did they would lose their identity and the justification for the reflexes in the first place would obviously disappear; (9) That reflex arcs are made up of a chain of neurones, which work together or fail to work together on the basis of the amount of resistance at their junctures or synapses; and (10) That on the basis of frequency and 'reinforcement' of some sort, aggregations of parts occur, which are the reflexes. These statements, unlike Hunter's,

² W. S. Hunter, *loc. cit.*, 455.

although logically implied by him, offer a more concise position to be verified or refuted by Lashley's facts. *Albeit that Hunter professes no love for the reflex arc concept, he denies the only other alternative position by his attack upon Lashley and by his categorical assertion that Gestalt, that is, a field property, is an empty concept. Accordingly he uses the notion of reflex action throughout.*

FAILURE OF REFLEX THEORY TO FACE NECESSARY PROBLEMS

By holding to the reflex theory Hunter has ignored the problems necessarily involved in a neural theory. Such a theory if it is to explain anything must tell us in neural terms how the underlying processes of observed behavior take place in the intact organism. In the first place behavior is unified in character. Behavior is not a concatenation of a number of individual things, monads, or processes scattered here and there, or reflexes in isolation, brought together under one label. Rather, it has properties that none of the parts, elements or reflexes possess, properties that cannot be derived from the mere summation of the abstracted individual responses. The problem that reflexology is trying to cope with is the deriving of this unity by aggregation or synthesis, a false problem for precisely the reason that unity already exists. In fact, unity must be assumed before any adequate theory can be formulated.

Second, animal behavior is adaptable in that the animal is not a system sufficient in itself, independent from physical conditions external to it. At the same time no one stimulus in the organism's external environment affects it to the exclusion of all others as the reflex school must, to be consistent, assert. If this were true the animal could never exhibit adaptability in the light of new situations or in fact in slight variations of the same situation. Hunter has admitted that the animal was adaptable in the case of the maze habit, but then contradicts himself by assuming a single stimulus-response situation in the brightness discrimination experiment. In all cases the animal is in equilibrium, throughout every sense department, with the external environment in keeping with the laws of dynamics.

Third, behavior is directional in that the end is established before activity takes place. It has a course, and in terms of the end or goal the phases of the course, which are oftentimes observed separately, are made meaningful. These phases which students of neural processes so often observe have made the reflex concept seem superficially to be sound, and in addition, have led to the belief that these phases exist in their own right. How could the directional character of these isolated events have been envisaged in light of the reflex concept? It is evident that they utterly failed to meet this issue, one of the most vital of all behavior problems. If such analyses of the process had not been so complete that the process no longer existed as we find it in the intact organism, this fallacy would have been apparent. Hunter seems to have missed the point entirely that a theory of the nervous system like Lashley's, based on field properties that obey the laws of dynamics, is the only type that ever avoided the logical and factual pitfalls of atomism.

Fourth, behavior is selective in that energy in all forms does not effect it and that equal amounts or similar patterns of the same form of energy do not effect it in the same degree at all times. For example, out of a total of almost four score octaves of radiant energy, the eye is capable of responding to approximately one octave.³ The organism possesses a dynamically balanced nervous system in terms of which it is not equally affected at all times by the same stimulus. If this were not true then we would expect just what the reflex concept logically implies, namely, that the same stimulus would affect the organism always in the same way. In an attempt to meet this problem of selectivity the reflexologists have further subdivided the reflexes into independent activities of cells, which worked or failed to work in terms of the amount of resistance at the synapse. On the basis of pure frequency or reinforcement these 'bonds' between cells were strengthened or weakened to produce this selectivity from one time to the next. By so doing the problem of behavior has

³O. L. Reiser, Contributions of the new physics to philosophy and psychology, *Psyche*, 1930, 11, No. 1, 71.

only become more confused. Whereas, before, the reflexologist faced the problem of aggregation of reflexes alone, he now has the added problem of accounting for unity within the reflex. The denial of unity as a necessary assumption for a neurological theory, such as the reflex school has made, was at the same time an implicit denial that selectivity could ever exist in terms of their theory.

Fifth, there is in behavior an observed expansion and differentiation of response; thus a theory must include the assumption of some sort of an expansion and differentiation in neuro-muscular organization. Wheeler⁴ calls this maturation and makes it the basis of learning. As such, it provides for the new in behavior inasmuch as the new cannot be accounted for in terms of the old. In terms of an equipment of discrete part activities, *each with its definite structural basis*, as is of necessity postulated in the reflex concept, expansion has no meaning other than that of a part of the organism acting at one time and perhaps the whole at another with no means of relating the part to the whole except by a fictitious structural accretion. Differentiation has no significance in a position that holds that behavior is a concatenation of reflexes or discrete units. Differentiation can only mean an emergence of details from a unified field, something which in itself the reflex school can never envisage, for in the postulation of a plurality it has been stalled in the futile attempt to gain unity from that plurality. Until the problem of unity becomes that of assuming unity instead of explaining it in terms of parts, the reflex school can never expect to do more than avoid the problems of behavior, such as adaptability, direction, selectivity, expansion and differentiation. Because the reflexologist cannot explain unity, with respect to its location in his system, it is for him logically non-existent. This then means that for him the behavior of the organism must be static in character, because dynamic qualities only adhere to a unified field. From what we know about the behavior of the intact animal, the most characteristic thing that can be said about it is that it is dynamic rather than static. If the

⁴ R. H. Wheeler, *The science of psychology*, New York, Crowell, 1929, 321 ff.

reflex theory cannot account for the most fundamental aspect of behavior, it has failed to fulfill its purpose. Since it does not face these problems it is not a neural theory.

MISCONCEPTION OF LASHLEY'S EQUIPOTENTIALITY

Hunter attempts to show that the concept of the reflex arc is more applicable to the facts under consideration than Lashley's theory of equipotentiality. In so doing he simply construes equipotentiality to mean 'a qualitatively homogeneous whole.' This statement is not only a misconception of Lashley, but also ignores the remainder of Lashley's theory, namely, that of mass action and functional equilibrium. In Lashley's own words his statement of equipotentiality is as follows: "the term equipotentiality I have used to designate the apparent capacity of any intact part of a functional area to carry out, with or without reduction in efficiency, the functions which are lost by destruction of the whole. This capacity varies from one area to another and with the character of the functions involved. It probably holds only for the association areas and for the functions more complex than simple sensitivity or motor coordination."⁵ And along with the notion of equipotentiality goes the theory of mass action which we shall also quote: "I have already given evidence (1927), which is augmented in the present study, that the equipotentiality is not absolute but is subject to a law of mass action whereby the efficiency of performance of an entire complex function may be reduced in proportion to the extent of brain injury within an area whose parts are not more specialized for one component of the function than for another."⁶ And to take care of his results further, Lashley makes the additional statement, "there is a considerable mass of evidence which suggests that some symptoms, particularly in the class of motor incoordinations, may result from disturbances in the functional equilibrium between centers, although no tissue essential to the performance of the dis-

⁵ K. S. Lashley, *Brain mechanisms and intelligence*, Chicago, University of Chicago Press, 1929, 25.

⁶ K. S. Lashley, *loc. cit.*, 25.

turbed activities is directly involved in the lesion. Thus unilateral lesions to the corpus striatum or to the cerebellum may produce marked disturbances of coördination although bilaterally symmetrical lesions involving the same structures produce but slight effects."⁷ From the above statements it is evident that Hunter has not only wrongly interpreted the concept of equipotentiality in calling it a homogeneous whole, but he has been unjust in his treatment of Lashley's position in that he has taken a part of a theory and attempted to contrast it with the 'total' current theory. But even if our objections are overlooked the 'total' neural theory that Hunter offers in contrast to his statement of Lashley's position, contains nothing that is relevant to the issue of equipotentiality *versus* homogeneous whole, and in addition gives us nothing that we could call a neural theory.

MULTIPLICITY *Versus* SIMPLICITY OF STIMULI UNJUSTIFIED
AND IRRELEVANT

Hunter's main argument has to do with the problem of simplicity of stimulus-situation as contrasted with multiplicity, a distinction that is unjustified in the first place and furthermore has no significance with respect to equipotentiality. Hunter affirms that in brightness discrimination a single stimulus controls the rat's activity, while in the mazes a 'multiplicity' of stimuli is involved. In the former he says that we know what controls the rat's behavior but in the latter, although there must be several types of stimulation occurring simultaneously, we do not know to what extent any one sense modality is active. Hunter has omitted the obvious fact that there is likewise a number of senses active in controlling the light discrimination problem, even though it is supposed that the rat responds differentially on the basis of brightness relationships alone.

Hunter interprets Lashley's difference in results between the brightness discrimination and the maze habits to be a demonstration of multiplicity of stimulation as against simplicity of stimulation, in discord with Lashley and pointless

⁷ K. S. Lashley, *loc. cit.*, 25.

with respect to the issue involved. Lashley's results seem to Hunter to demonstrate a localization of function in the light discrimination problem and not in the maze problems. This was, he said, explainable on the basis of multiplicity of stimuli in the latter as contrasted with a single-stimulus response in the former. By implication the whole brain was active in the maze problem and simply a part in the brightness discrimination experiment. And thus the one type of behavior must have been more simple neurologically than the other. Hunter denies that the overt behavior was any different in the two cases. The motor responses were the same; they were as complex in the one case as in the other. But this then contradicts the statement that the one type of behavior was more simple than the other, for presumably it would take as many motor reflexes for the rat to progress along an alley in the one case as in any other similar case. We do not know what the rat smelled in either instance. It could have smelled the same things, but in both cases it was obviously smelling. It was also sensitive cutaneously, kinæsthetically and viscerally. All this Hunter admits. It is presumptuous to throw away the activity of the senses other than vision in the brightness discrimination situation, for it cannot be said that the rat did not need motor organization to reach the goal! Brightness discrimination was not all that there was to the behavior. The motor organization that was necessary only had significance in functional relation to vision. *In fact no one has been able to distinguish neurologically which is the one and which is the other. In an intact animal no one ever demonstrated the separateness of the senses except anatomically.* We are not talking about anatomy but are trying to obtain a picture of behavior and thus must deal with dynamics, a problem that Hunter refuses to face.

That the behavior of the animal is dynamic in character with respect to the goal, and not controlled by one sense department may be seen first in the fact that this motor organization with reference to the goal was by necessity not the same from trial to trial, for the animal found himself in at least slightly different positions from time to time. Lashley

very definitely observed that there was a lack of stereotypy in the behavior of his animals. Others have seen this as well, when, in one trial the animal would dash through the alleys, 'cutting' the corners, and in another would hesitate or take a devious route. This is not only true during the learning process but also after 'perfect' runs are being made. There must then be the failure to use just quite the same galaxy of reflexes from time to time. If the animal's movements were not in harmony with the so-called brightness discrimination, the animal would never proceed toward the goal. And if this lack of harmony occurred would we not say that the habit of discrimination had been destroyed? Hunter offers no theory that will account for the organization of these variable responses, in his use of the reflex concept. The absurdity of the association hypothesis, upon which the reflex arc theory is based has long since been demonstrated.⁸

On the other hand, as well as showing that one cannot single out any particular sense to the exclusion of all the others and call the behavior a one-stimulus response, one cannot show that in the case of Hunter's multiple-stimulus response the subtraction of a given sense will cause the dropping out of discrete bits of the maze habit corresponding to a given sense field. For example, in as much as movement is involved in the maze habit, one might expect that the complete destruction of the motor cortex would eliminate the maze habit by making the animal unable to move, but this is not the case, for the habit survived this destruction and also that of the corpora striata. Furthermore it may be pointed out that in monkeys manipulative habits survived extirpation of both precentral gyri. Thus Lashley concludes that, "retardation of learning and the loss of the habit from cerebral lesions cannot be ascribed to defects of the motor mechanisms as such but involve disorganization at a higher level of integration."⁹ It is plain from the above evidence that the mere subtraction of the motor cortex does not abolish organized movement

⁸ See K. Koffka, *Growth of the mind*, New York, Harcourt, Brace, 1924, and R. H. Wheeler, *loc. cit.*

⁹ K. S. Lashley, *op cit.*, 116.

required to run the maze, as would be supposed on the basis of the correlation between structure and function postulated by the reflex school. This same principle operates just as effectively with respect to the rôle of the various senses.

Second, Lashley, contrary to Hunter's implication, did not ignore the possibility of discrete sensory components responsible for the animal's behavior, so that if certain brain 'insults' were made, certain sensory functioning might, in a more or less clear cut way, cease. He tested the rôle of several of the senses in maze running and in conclusion states, "the analysis of the maze habit in relation to the cyto-architectural areas of the cortex failed to reveal a constant dependence of the habit upon any single field, as might be expected if the habit were dependent upon any single sensory function."¹⁰ Lashley blinded animals and found no significant loss in maze habit due to the blinding. Likewise he blinded rats before learning the maze habit and then destroyed portions of their cortex (occipital) after learning and found effects similar to those obtained when the seeing animals were subjected to occipital extirpation after learning. He likewise made reference to Munk's reports in which it is stated that dogs blind from occipital lesions have difficulty in finding their way about whereas those blinded by section of the optic nerve readily adapt to the altered conditions. This demonstrates the equivocal relationship between any of the classical topographical areas and the elimination of any given sense.

Both Hunter and Lashley cite Watson's work in the attempt to determine the rôle of sensory components. Neither of the three can deny that the animal does use a given sense when equipped with it, just because the animal still reacts when deprived of it. Lashley makes this functional analysis of the senses to determine if the lack of any one of them would cause the rat to be unable to run the maze. And in finding that the lack of any sense does not destroy the habit, nor at times greatly alter it, he could then relieve himself from ascribing any loss in the maze habit to the loss of specific

¹⁰ K. S. Lashley, *op. cit.*, 110.

sensory capacities. He not only succeeded in going that far in supporting his position, a point which Hunter seems to have missed, but carefully noted the nature of the behavior of the injured animals to be that of general deterioration. There was no evidence of a dropping out of some discrete element, nor did elements in a series of movements drop out and leave the animal stalled somewhere as would be expected from the reflex arc standpoint.

The work cited above has shown conclusively that not only is the maze habit, as Hunter asserts, controlled by a multiple stimulus situation, but the brightness discrimination as well. Thus the distinction which Hunter makes as his main point is abortive. The work of Lashley and Watson shows that one cannot predict from the dropping out of sensory components what the resultant behavior will be. Summation as Hunter employs it in making his simplicity-multiplicity distinction fails to show up in these experiments, inasmuch as one cannot strictly determine the rôle of any of the senses in the intact animal by their elimination from an animal experimentally. Thus the summation or subtraction of sensory components is futile for purposes of prediction.

AVOIDANCE OF FACTS OF RELEARNING AFTER OCCIPITAL DESTRUCTION

Among the features of Lashley's experimentation which have to do with equipotentiality is the problem of relearning after cerebral injury, which is totally ignored by Hunter. If Hunter had considered this aspect of Lashley he probably would not have been so insistent upon an answer in explanation of the difference in results between the brightness discrimination and the maze problems. Hunter does not go all the way and consider why the discrimination habit can be relearned by the occipitally extirpated rats as readily as it was learned by the normal rats in the first place. Should one ask Hunter to answer this query, one would find that reflexes would not account for it. Should the answer be that a new set of reflexes were formed in the tissue of the cortex that was left, then immediately the alternative view of some degree of

equipotentiality would essentially be admitted. Structure and function could not coincide if this be true, for according to strict reflexology the tissue that remains is for other functions and if it takes over the functions of the destroyed areas it is serving two functions. This in its simplest form has been termed vicarious functioning and is an abandonment of the crudest notion of the correlation between structure and function. It is not necessarily a forsaking of atomism, however, and until that is done no theory fails to raise more problems than it solves. One cannot predict then from structure to function if there is no strict correlation and if in addition one attempts to view as parts that which is unitary, namely, the nervous system. If we see it atomistically, we see it as it never occurs in the intact organism and in that way our theories go astray.

IRRELEVANT AND UNJUSTIFIED DICHOTOMY OF 'CENTER' AND 'PERIPHERY'

Hunter assumes, irrelevant to the argument of his article, an age old dichotomy, namely, that of dealing separately with 'center' and 'periphery,' in using independently central and peripheral explanations. With reference to such he states a maxim, that it is wise never to use a central explanation when a peripheral one will do. When in psychology has a peripheral explanation ever sufficed? Have peripheral concepts sufficed in theories of audition as Hunter suggests? As an admission that they have not we find Forbes,¹¹ Wever and Bray¹² and others hard at work on the nature of the differential processes in the auditory nerve and in the brain stem, both more central than receptor processes or ear anatomy. In so working they are dealing with problems of greater significance to the behavior of the organism-as-a-whole than peripheral details. Even though we do not yet know the details of the mechanics or the hydraulics of the inner ear, we can well afford to relegate such investigations to a secondary

¹¹ A. Forbes, R. H. Miller, and J. O'Connor, Electrical responses to acoustic stimuli in the decerebrate animal, *Amer. J. Physiol.*, 1927, 80, No. 2, 363-380.

¹² E. G. Wever, and C. W. Bray, Auditory nerve impulses, *Science*, 1930, 71, 215.

place and give precedence to the study of neural functioning, so that we shall no longer hear of 'perceptive powers' of the auditory nerve as we did several decades ago or stop at 'auditory reflexes.' The fading of the visual purple might explain something, but it does not explain the dynamics of the individual's behavior as we find it. We stop at visual purple only if we are chemists interested in photochemicals, but not as psychologists.

Reflexology makes it necessary to refer to the brain for the integrative function. When Hunter advises against 'central theories' he leaves the reflexes non-integrated or disintegrated. It is only by violation of his own maxim that he can get them integrated, for one always has to refer to the central nervous system for this integration. If it is always necessary to include the center with the periphery, wherein lies the need of the maxim?

ENGRAMS EXTRANEOUS AND USED INCONSISTENTLY

Forsaking still the thread of the argument Hunter attacks Lashley's concept of engrams with the contention that they are central. But on the other hand Hunter declares that neural engrams are the only mechanisms that can be resorted to for explanation in some of his as well as Gellermann's unpublished work, at the same time forsaking the simplicity-multiplicity control of behavior. What we evidently have in the simplicity-multiplicity control is not a general theory that can be applied to all behavior, but one that must be excluded in face of a real problem. At the same time he refers to 'symbolic' processes as necessary to help explain his own results. With a neural engram that is central and the symbolic process that is peripheral, Hunter burdens himself with a new dualism. In addition to this in employing the symbolic processes, he constructs another dualism. Ordinarily one thinks of a symbol as something apart from and representative of something else. Now if symbolic processes are of such a nature, we have a symbolic process and along with it a real process which does the work.

THE MEANING OF *Gestalt* MISUNDERSTOOD

Hunter makes reference to the notion of *Gestalt* calling it an 'intriguing but empty concept' and without further clarification leaves the argument, satisfied that it is clinched. Possibly the reason for this is that if the word means anything at all in his theory it is only a synonym for integration. That may be how it becomes 'empty' for him. In that case we too should call it empty. Inasmuch as any word means what one makes it, it is advisable to look not only to the word itself but to the definition it has by usage and its logical implications in a theory. Undoubtedly the word has passed the lips of too many and is like new wine poured in old bottles. The word means nothing and adds nothing in an atomistic system or in systems holding to bundles of reflexes or any other elements. Hunter makes *Gestalten* central organizations and thus for that reason if for no other dispenses with them in keeping with his maxim. But the concept of *Gestalt* does not deal exclusively with central factors any more than it deals exclusively with peripheral, rather it assumes that the distinction between central and peripheral is logically unjustified and untenable. It is true that the concept is not some central mystical *sine qua non* to which one can apply all facts not to be explained in any other way, which is just what Hunter seems to be searching for. It deals with the behavior of the intact organism as a unitary process and not one to be broken into imaginary discrete bits of experience. With this in view the concept of *Gestalt* ceases to be empty and has the entire science of physics to support it.

SUMMARY

We have demonstrated that Hunter's attack on Lashley's theory of equipotentiality in the form that it has taken has been entirely extraneous and futile to the issues in question for the following reasons: 1. His statement of the current neural theory is noncommittal and too brief to form any contrast to Lashley. 2. The failure of the reflex theory, which Hunter upholds, to face the fundamental problems of neurology eliminates it from the category of a neural theory at

the outset. Chief among its failures is its inability to cope with the origin, direction and culmination of any given response. 3. Hunter has misinterpreted and used unjustly Lashley's theory of equipotentiality by making it mean homogeneity and by pitting it, which is only a portion of Lashley's theory, against the whole of the 'current neural theory.' 4. As the main point in his argument Hunter attempts the unjustified distinction of multiplicity *versus* simplicity of stimuli as an argument against equipotentiality. This is entirely foreign to the argument, for the reason that it has nothing to do with the adequacy of current theory. 5. Hunter has avoided mentioning relearning after cerebral injury, one of the main facts taken care of by Lashley's theory and one for which current neural theory offers no solution. 6. The dichotomy of 'center' and 'periphery,' since it has been brought in, although irrelevantly, by Hunter, has been dealt with and shown to be unjustified. Here also his maxim has been demonstrated to be useless inasmuch as the only integration mentioned is 'central.' 7. Hunter has been inconsistent in denying the validity of the neural engrams in Lashley's theory, while he resorts to them in explaining some of his own work. This too is extraneous to the contrast between 'current neural theory' and equipotentiality, as treated by Hunter. 8. Hunter's implication that *Gestalten* are central processes is an admission on his part that he fails to understand this conception. The reason is obvious. Hunter may believe that a reflex theory can be expressed in terms of dynamics and doubtless he would argue that he agrees with much of what we have said. But in this he would be inconsistent, because the dynamics of a situation are of necessity field properties and not reflexes, a fact which the *Gestalt* conception, for the first time in psychology, recognizes, and which Hunter denies. It should be obvious, of course, that Lashley's relative equipotentiality and mass action pertain to field properties.

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CONDITIONED REFLEX THEORIES OF LEARNING ¹

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The theories which conceive of the learning process in terms of the conditioned reflex or the conditioned response have received a great deal of destructive criticism recently, some from those who understand the nature and assumptions of these theories, and a great deal more from those who apparently do not. Under the circumstances, it seems desirable to present once more the general position of the behavioristic and near-behavioristic schools on this particularly important point, before attempting to analyze and answer some of the more significant criticisms of it.

The assumptions of the conditioned reflex theories may be grouped under two headings, which for convenience may be called psychological and neurological. The psychological assumptions have to do with the explicit development of learning processes, the neurological, with the physiological structures and processes underlying.

The term reflex is construed to mean any process which occurs prior to the occurrence of learning.² It is, therefore, inclusive of all native forms of response, not, as some of its critics have suggested, limited to glandular responses and spinal arcs. The human response repertoire is consequently composed of two forms of behavior: reflexes and habits. The former represent the units from which the latter are constructed, since the behaviorists take the position—now apparently antiquated—that every process has a logical antecedent in some other process. Inasmuch as reflexes are the entire components of true innate behavior and all acquired

¹ Presented to Dr. Henmon's seminar in psychology, University of Wisconsin, Dec. 12, 1929. Dr. Henmon and others have kindly offered suggestions and criticisms, but are responsible for none of the opinions here set forth.

² We pass over here as irrelevant to our problem the important questions raised by Dr. Kuo on inheritance.

responses are based upon them, to talk of different 'levels' of behavior implies a contradiction in terms. The use of the word 'intelligent' would be limited to equivalence with the present word 'adaptive'. There is no supernatural force, entelechy, consciousness or mind, which is capable of changing the normal course of events and influencing the learning process.

Reflexes are called forth in the first place by certain particular stimuli. To assume that, for effectiveness, they must influence identical receptor elements every time, is purely gratuitous, a point which we shall consider when we come to neurological factors. For the native or 'unconditioned' stimuli, new ones may be substituted by the process known as 'conditioning'. The reflexes may be combined into series (unfortunately called 'chains') in varying orders which are determined by the sequence of their respective stimuli and often are supported by proprioceptive stimulation from the performance of preceding members of the series, as in walking. Both the process of conditioning and its product, the conditioned reflex, as representative of the learning process and the learned response, are under fire at the present time.

Before going on to the question of the neurological assumptions involved, let us consider a typical example of this type of learning. In order to include what the average man would call 'intelligent' behavior and at the same time demonstrate the common relation with conditioned response mechanisms, we may legitimately use the example of a child learning to read. We have here certain visual pattern stimuli, letters, arranged in patterns to form words. The child must first form the association between the appearance of a given visual pattern in the environment and the motor response of pronouncing the word. Before this can take place the child must have acquired the response of reproducing sounds stimulating it from the environment. Thus the laryngeal reflexes are conditioned auditorily first, and the conditioning to a visual stimulus is conditioning of the second order. A successful response to the visual stimulus brings rewards in the form of approbation or candy. This approbation is the only criterion

of correctness for the child. Errors in pronunciation are unrecognized, and will be perpetuated if not corrected. There is no intrinsically right response; the reward determines which shall be conditioned. The letters and words presented have no more real relevance for the child than have the squares in a Yerkes box for a rat, and the behavior is often strikingly similar: as when the child reads with his book upside down, or the animal reacts to position cues. The *response* is the same for correct and incorrect behavior; it is the *stimulus* which determines when the response will be fixated and when it will be eliminated (suffer experimental extinction). As the child learns the relation between visual stimuli (words) and the laryngeal contractions necessary to their pronunciation, he is continually being rewarded by success in manipulating his environment. This reinforcing process never stops. Indeed, when it has been suspended over a long period of time, as when one spends some years in a foreign country, the native tongue may be half-forgotten or almost entirely lost.

How does this conditioning process take place in the nervous system? The assumptions of the behavioristic theory are relatively simple. All responses of striped muscle, smooth muscle or glandular tissue consist of reactions to stimulation from motor or efferent nerves. The connection is morphological, therefore inherited. This class of behavior comprises what are called unconditioned reflexes. Certain energy changes in the environment act selectively upon the nervous system and activate specific groups of these effectors; these are called unconditioned stimuli. Other energy changes activate all the response mechanisms to a minute degree; these are indifferent stimuli.

An indifferent stimulus activates an afferent neuron. It sets up an electrical disturbance in the nervous system, eventually in the cortex. At the same time an unconditioned stimulus activates its selected response mechanism, and also sends an electrical disturbance to the cortex. If, as recent studies in neurology seem to indicate, a disturbance of and return to electrical equilibrium marks the passage of an action current, then between the two activated points in the cortex

there will be a difference in polarization, and probably in potential as well. A circuit will be completed through the non-medullated fibers of the cortex. Note that this need not be confined to any single neuron. In an area such as the cortex, whole fields rather than individual cells must be the functional units. Eventually, in any case, the activation of the indifferent stimulus pathway will lead to activation of the unconditioned stimulus pathway, with the resultant response following.

This abbreviated sketch of the learning process may probably be considered as approximately representing the behavioristic view on the subject. It is not perfect, and it is schematic rather than detailed. It will serve, however, as a basis for discussion of the criticisms of conditioned reflex theories, especially neurological criticisms.

From the psychological point of view five points have been adduced to show that the conditioned reflex is unsuited to explain human and animal learning. As usually given, these are: (1) the variability and impermanence of the conditioned reflex, and the number of repetitions required to establish it; (2) the over-simplification of the concept; (3) the dying out of all reflexes after some time; (4) the readiness of experimental extinction; and (5) the rapid onset of abnormal conditions and pathological states. Most of these are based directly on the experiments of Pavlov (15). My answers, however, will largely tend, not to refute these objections, but to show them fundamentally characteristic of animal learning, and of early human learning, before the establishment of so many conditioned stimuli to the same reflex system makes analysis difficult or impossible.

(1) Pavlov reports that several trials are required to establish a given reflex response, that they vary considerably from trial to trial, and that they may disappear overnight. In human studies on reflex responses this has also appeared. Watson (19) reports one subject who, without practice from May to October, showed a steady response after a single application of the unconditioned stimulus. This is a single case and must be discounted as such. But is this fact of slow

acquisition, fluctuation and rapid loss so uncharacteristic of human learning? Consider the example already given, of the child learning to read. There will be days of encouraging progress, and days of apparent utter stupidity.³ A passage which is negotiated successfully one day may be hopeless the next. Until the child has been practising for years, he will be uncertain and full of vagaries. The characteristic curve of retention for adult subjects confirms this statement as to the sudden loss of acquired material. It seems that we may legitimately consider this so-called criticism a point in favor of comparing learning to the conditioned reflex.

(2) As has already been pointed out, it has been the custom for critics to set up their own definition of a reflex and then attack the behavioristic school on that ground. Native behavior is unquestionably composed of 'muscle-twitches' and glandular secretion. It is still capable of variability in accordance with the requirements of a given situation. Thus a strong stimulus will call forth a greater amount of secretion than a weak one (abnormal cases excepted) and differentiations of a very exact sort can be set up, as when we learn not to reach for something behind a plate-glass window, although the difference in the visual field may be very slight.

The behaviorist has been regularly accused of treating habits as built up out of isolated small movements, like so many bricks. This is hardly fair, since Watson (20) pointed out in 1919 that the child employs relatively gross movements at first, later coördinating the finer muscles. In the adult as well, habit proceeds from crude to accurate adjustment. Consider the description: "The conditioned motor reaction is usually sharp, quick and widespread, the whole body as a rule being brought into the reaction at first. Gradually the reflex becomes more circumscribed" (19). This parallels the general law of growth which Hollingworth has characterized as 'from fundamental to accessory'.⁴

³ For a detailed report of a child's learning color names, see W. Preyer, *The senses and the will*, New York, D. Appleton & Company. The above point is illustrated to the satisfaction of all.

⁴ *Mental growth and decline*, Appleton, 1929. It is to be noted that unconditioned reflexes develop in the same way. Cf. G. E. Coghill, Genetic interrelation of instincts and reflexes, *Psychol. Rev.*, 1930, 37, 264-266.

While the above criticism is true to the extent that behaviorism considers each reaction as the sum of its part-reactions, and no more, it is not legitimate to make the statement that whole reactions are ignored. We may again quote Watson (20) on this point: "*The behaviorist is interested in integrations and total activities of the individual . . . we do not stop as a rule to reduce the total activity to muscle twitches. We can do it if necessary and we do do it when it becomes necessary to study the various part-reactions.*" This emphasizes the integrative aspect, yet it does not imply more than a summative, as contrasted with an emergent, view of the unity of organic reactions. If the behaviorist is told that a whole is more than the sum of its parts, he will immediately ask such questions as, Can the difference be measured? What is the source of this excess? On being told that the difference is of an intangible sort, which cannot be studied scientifically, and that the whole just *is* greater, without any assignable reason, he will soon lose interest, because *the behaviorist is interested in the prediction and control of human behavior*, ends which demand the measurement of phenomena and assignment of reasons for them.

This attitude may be defended, not alone on pragmatic grounds as being the only workable scientific attitude, but by analogy from other sciences. The botanical theory, if we may call it such, which endowed each tree and plant with a spirit, was a view much less productive of practical knowledge about plants than that theory which ascribes the same phenomena to cell division and photosynthesis, even though we may be far from a real understanding of the latter. Meteorology did not progress very rapidly as long as the weather depended upon the temper of some supernatural individual. Looking back upon such states of affairs, we can hardly blame the behaviorist for looking among tangible, measurable aspects of phenomena for the sources of human activity.

When we consider any complex activity, such as riding a bicycle or driving an automobile, it *seems* to be made up of various part-reactions, and each of these seems to have some relation to the stimulating situation. They are probably

conditioned reactions. Cason (3) has written an excellent review of the literature on the subject of the conditioned response mechanism, and concludes that it is a characteristic of living organisms in general.

(3) Pavlov reports that even in the most stable of his experimental animals, all of the conditioned reflexes ultimately passed into the inhibitory stage and became useless. There was very wide individual variation in this respect, and it was also noted that the establishment of new reflexes served to resuscitate the old for varying periods of time. Introduction of extraneous stimuli (disinhibition) also served as a therapeutic measure in these cases.

Most of us are constantly learning new things, new responses. That is, we are conditioning new reflexes, or grafting new stimuli onto the old. We are also regularly being subjected to variable and novel stimulation of a sort calculated to bring about disinhibition. Sometimes it is even strong enough to bring about external inhibition, as when the passing of a band under the window stops all work for the time being. These two factors seem to be adequate to insure the permanence of responses which have been firmly conditioned.

(4) The laboratory studies of the conditioned reflex are unanimous in emphasizing the rapidity of disappearance upon non-reinforcement. The reflex may, however, reappear spontaneously after a lapse of time, or a single application of the unconditioned stimulus will reestablish it. The best example of this phenomenon that occurs at the moment is the method of curing children of temper tantrums, a form of behavior in which the rage response is conditioned to any 'blocking' situation. It is generally agreed that the best treatment for this behavior is to leave the room and allow the tantrum to run its course without attaining any reward of attention or some desired privilege. A few treatments of this type are wholesomely effective in eliminating the objectionable tantrums. It is also easy to observe, in difficult learning problems, the onset of experimental extinction at about the phase usually called the plateau. If the individual is not making the progress which he thinks he should, there may

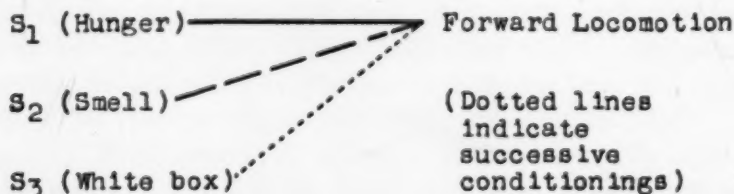
be an actual decline in efficiency (I have noted this with particularly hard nonsense syllables). The habit may be lost entirely if additional incentives in the form of verbal encouragement are not offered. The success of Dunlap's method of habit-breaking by repetition is due apparently in large extent to the experimental extinction of the incorrect response. In general, it will be true that very few of our habitual responses will be endangered by experimental extinction, because they have become habitual by virtue of being adaptive, hence they will not go unreinforced.

The elimination of wrong responses, *e.g.*, in a maze, bears a striking and more than superficial resemblance to experimental extinction. The rôle of this process in cutting out unessential response elements from a situation has been mentioned by Hull (9). Its importance in the explanation of trial and error learning he has discussed further in a recent paper (10).

(5) The work which Pavlov has reported on neuroses and pathological disturbances as a result of functional interference indicates that the reflex is very delicate and subject to immediate influence of an abnormal condition in the central nervous system. Thus strong stimuli lost their effect, while weak stimuli became quite potent (paradoxical phase). Cases of hysterical anesthesia and hyperesthesia illustrate effectively the applicability of this phase of conditioned reflex work to the human field. In a more nearly normal case, we may cite the mother, whose ear is attuned for the slightest cry of her child, yet may be quite insensitive to other things. This is not to be confused with the everyday phenomenon of selective attention. Selection of items to be attended to is largely learned and depends fundamentally upon experimental extinction. The pathological work of Pavlov's school shows a deep-lying similarity to functional abnormalities in the human being, and seems to be essentially another argument for the appropriateness of the conditioned reflex theory of behavior. That the phenomena reported have their parallels in the physiology of nerve fiber was found by Sherrington and Snowton (see 5).

Williams (21) has attacked more fundamentally the conditioned reflex theories. She calls into question the fundamental adequacy of the concept of conditioning as being comparably similar to the facts known about simple learning situations. That the conditioned reflex is itself a simple learned response and therefore unquestionably has some right to be compared with other learned acts, seems not to have occurred to her. We may consider her example, which consists of the learning of a discrimination habit by a rat: "If the approach to the white compartment is a case of conditioning, it is also something more. The conditioned reflex formula takes account only of the unconditioned stimulus, the conditioned stimulus and the response. There is no room in it for S_2 and R_2 , the consummatory phases (eating, in this case), without which the conditioning . . . does not occur. . . . In this case, then, there is no response made to any unconditioned stimulus whatsoever with which the conditioned response can be considered identical. The conditioned response scheme is, therefore, inapplicable."

On the contrary, it seems to me that the conditioned response scheme is the only one applicable. In the first place, however, smell of food is not an unconditioned, but a conditioned, stimulus for approach. Locomotion surely is a reflex activity. Then the following scheme represents the state of affairs:



Hence, at the same time, the corresponding response to the black box will undergo experimental extinction. Then the one stimulating situation will be a conditioned stimulus for approach, the other for non-approach, and a conditioned inhibition has been set up. The permanence of this conditioning depends upon the thoroughness of the conditioning of S_2 which in the average adult rat is very thorough indeed.

The conditioned reflex experiments themselves, as made on human subjects, have been the center of much controversy. The chief point in these arguments has been the resemblance of the conditioned reflex to the so-called 'voluntary' reaction. Ultimately, of course, the two are identified by behaviorism. For the purposes of demonstration, however, it has been necessary to set up conditions in which 'volition' cannot affect the results of the experiment. Typical of these is Cason's work on the conditioned pupillary reflex, which is not subject to voluntary control. Watson's work on babies is well known, but others have reported interesting data as well. Skerrett (18) established a fear response to the odor of a rose at 7½ months. Krasnogorski (see 3) reports the sucking reflex conditioned to the sight of milk at 14 months. Mateer (see 3), using Krasnogorski's technique, tested 67 children by the conditioned reflex method, thus proving that it is not incompatible with the American tendency toward mass experimentation. Allport has taken the principle as a fundamental in explaining the development and socialization of the individual.

Aside from the objections to conditioned reflexes as explanatory of psychological phenomena, there is a considerable feeling in neurological circles that interpretation of cortical activity in terms of specific stimulus-response 'arcs' is unjustified. Conditioned reflex theories in general require some sort of pathway or 'engram' which is retained in the sense of leaving an actual physiological change in the neurons affected. They therefore require a certain specificity of neural function, as opposed to the concept advanced by Lashley of mass-function. In the case of certain types of stimulation, it is also necessary to interpret them as involving a point-to-point correspondence between receptor-cell and cortical cell. Each of these concepts has been the center of considerable discussion recently.

The assumption of a point-to-point correspondence between the visual analyzer and the cortex is unnecessary, on account of the structure of the retina. It will be remembered that in the second retinal layer, just in front of the sensitive

visual elements, there occur a great number of the cells known as Golgi's Type II. Herrick (7) describes these as follows: "The axon is very short, breaking up in the immediate neighborhood of the cell body; these . . . appear to be adapted for the diffusion and summation of stimuli within a nerve center." It thus follows that a stimulus, *e.g.*, a visual pattern, falling upon the retina, may conceivably affect the cortex by any number of paths. These Type II neurons offer possibilities of multiple connection to an unlimited degree. 'Recognition of visual patterns' is therefore not necessarily a matter of *Gestalten*, but is a function of retinal structure, since an influence falling on one retinal element will be partly changed by influences falling on other retinal areas.

The results of Pavlov with the tactile analyzer give very convincing evidence for some sort of point-to-point correspondence here. A conditioned reflex was established to a tactual stimulus and generalized for the whole body, before removal of the *gyri coronarius* and *ectosylvius anterior* in the left hemisphere. On the eighth day the reflex returned for the left side of the body, and on the tenth for stimulation of the middle part of the right. There was a very sharp line of demarcation between positive and negative areas. The reflexes for the shoulder and pelvic areas, fore and hind limbs, did not return until over 90 days after the operation. Since this was a generalized reflex, relearning had probably occurred in that amount of time.

It has been argued that these are mere sensory functions, and that the behavioristic theory implies localization of memory, intelligence, imagination and other complex functions. This doctrine has never been enunciated by any behaviorist with whose writings I am acquainted. Faculty psychology and phrenology are as little in accord with this theory as with structuralism. Indeed, all general theories of learning on the basis of the conditioned reflex must involve the activity of the entire cortex, since there is no phenomenon of every-day existence in modern times which does not require a complex interplay of stimulation through almost every receptor, plus the usage of physiological traces of past experi-

ence. I believe, therefore, that those neurologists who argue against the conditioned reflex on the basis of its neurological assumptions, are guilty of unfair interpretations of the theory.

It is certain that there is some degree of localization in the normal cortex, and that therefore the different components of a given habit would have relations to certain areas on that basis alone. The localization of sense function, while not accurate, is reasonably clear, when we consider the wide ranges of individual differences in capacity and training, and the fact that the convolutions which are used as landmarks do not occupy fixed locations in different individuals and species. Campbell (2) says that the cyto-architectural divisions of the cortex are considerably more numerous than the gross morphologic divisions known as lobes. Piéron (16) suggests that these may be the true basis of functional localization, and there is no reason why this should not be the case.

From the logical aspects of the situation we should expect *some* localization of function. The neo-pallium represents a fairly recent evolutionary contribution. In the rat it is rather diffuse and, as compared with man, small. The sub-cortical structures are still of the highest importance. In the dog we find increasing development in area and complexity of the cortex, and in the primates this is even more marked. In accordance with the general evolutionary principle of progress by specialization, we would consequently expect more and more definite localization of certain functions which are capable of being localized. This is in fact true. Extirpation of the cortex of the rat, even of the whole occipital lobes, did not necessarily result in permanent scotoma (11). Pavlov, on the other hand, reports that after bilateral extirpation of the occipital lobes, none of the dogs ever showed any sign of object vision, though some were kept for years. These animals could react to changes in illumination, however, which Pavlov interprets as meaning a slight extension of the visual analyzer beyond the occipitals. Rothmann (17) reports a case of a dog which never regained auditory function after

Careful extirpation of both temporal lobes.⁵ Unfortunately, no controlled experimentation is available on human subjects, the sources of information being limited to pathological cases, in which oftentimes we do not even have the opportunity of a necropsy to determine the precise location and extent of the lesion.⁶

Recent experiments by Lashley (13) are made the basis for a rejection of the conditioned reflex type of neurology. Head (6) concludes his study of aphasia by concluding against the *exact* localization of 'psychic' processes in the cortex, but finds a *general* localization and points out logical reasons for this state of affairs. Both these criticisms of the conditioned reflex theories are unfair in that they interpret too strictly the term 'localization.' Cortical specificity cannot apply to functions which have no specific anatomical basis; *e.g.*, intelligence could not be localized, because intelligence is a function of the whole organism.

Lashley's experiments were apparently conducted on the theory that a maze habit could be bottled up in a single cell or group of cells, or else that continuity of a given neuron path would be essential to the running of the maze. Such a theory would not harmonize with the conditioned reflex hypothesis. The maze habit is one involving most of the exteroceptors and probably even more of the proprioceptors and interoceptors of the animal's body. We would expect, therefore, that the cortical pattern representing a learned maze habit would be a very complex one indeed, involving points of activation in all of these centers. If he had removed all of these, he would have been under the necessity of removing practically all of the cortex, a process which would involve other effects than loss of memory. If he removed a few of these loci of activation, he would slow the functioning of the habit proportionately.

⁵ The criticisms by Johnson of Rothmann's technique cannot be ignored in this connection. Pavlov's results on the same operation are more reliable and should be given more consideration.

⁶ A striking case in which the visual area was determined in a human brain without operative lesion is that of Laura Bridgman. Donaldson was able to determine the limits of the visual area with great accuracy in her case, because that portion had suffered selective structural degeneration.

His results are also vitiated by the fact that operative shock is inadequately controlled. In the first place, his normal group suffered no operation at all. The only adequate control in this case would have involved anesthesia and opening of the cranial cavity. In the second, he himself gives evidence indicating the significance of operative shock when he points out that one of his rats, in which two operations were given to remove a certain amount of cortex, suffered much less retardation than those in which a single operation was used. Evidently a removal of a large amount of tissue suddenly disturbs the functioning of the system even more than losing it gradually.

To interpret his results as indicating absolute non-specificity is, it seems to me, to read in a great deal which is not given in the evidence. The brightness habit, for example, was lost when the occipitals were removed. On the other hand, he is fundamentally right in insisting that the brain is a dynamic system, in which any disturbance of balance produces erratic and abnormal responses. This might remain unnoticed in a simple function, but in complex processes would show very clearly. This does not prove that the parts of the cerebrum have no specific functions. We do not expect a hand to work as well with three fingers as with five, but we nevertheless admit that the separate digits may participate in characteristic individual responses.

Lashley (12) has also attacked the localization theories by way of studies on the localization of motor points in the pre-central gyrus or motor area. He found that there was a considerable variation in motor response to the stimulation of the same point from day to day. From the point of view of Lapique's chronaxy theory this is not so difficult of explanation. Very slight variations in the intensity of the galvanic current employed, or in the time of stimulation, might have caused different Betz cells to be stimulated, with correspondingly different spinal efferent tracts activated. The rôle of the Betz cells is of great importance here, as Hoffman (8) has reported. Direct stimulation of the spinal tract transmits the frequency of stimulation to the muscle fibers unchanged,

but if stimulation takes place through the cortex, the frequency of contraction will be independent of that of stimulation, and will depend on the pyramidal tract being activated.

It is hardly enough, however, to criticize the opponents of cortical specificity. We must give positive evidence as well. Mention has already been made of the work on extirpation of parts of the cortex in different species, and of the clinical work of Head, which gives more data favoring than opposing specificity. In the recent work of Piéron (16) will be found a summary of the best research on cerebral localization since Panizza discovered the visual center in 1856, and also of the results of pathological studies during the war, in which lesions of almost every type occurred for observation. As an outcome of the latter work, he feels safe in concluding that "the function of speech is isolated in a single hemisphere, in the area from the posterior part of the parietal and temporal lobes to the level of the angular gyrus as far as the foot of the frontal convolutions." With injuries in this area, correlations of a fairly exact sort can be found between location and type of disturbance. Thus lesions of the parietal areas included in the speech center result in loss of power to recognize tactual objects. Voluntary control of movement may remain, yet the power to execute habitual movements, such as the sign of the cross, is lost. Lesions of the occipitals may not cause scotoma, yet written words may lose all meaning. In general certain symptoms, characterized as 'atopical' because they had no anatomical correlate, corresponded to the symptoms of trauma, while the remaining syndromes had quite definite positions in the speech center.

It is interesting in this connection to note that Cushing (4) reports stimulating the motor area of a patient during consciousness, and the resulting movement was reported as occurring voluntarily. Thus the behaviorist may feel justified in saying that we are being duped by our own neurons, which respond to stimuli in a perfectly automatic manner, yet pretend to do these things at our bidding.

Considering the limitations of clinical work on man, the

developments mentioned above are indeed encouraging. Too much must not be concluded from them, however, and Piéron does not forget to anticipate the accusation of returning to phrenology: "The impossibility of actual entities, like attention, memory and intelligence, will be realized. . . . Intelligence is a value-judgment which we pass on the functioning of a cerebral machine." This attitude seems to be highly appropriate in the absence of further knowledge. Localization is of separate reflexes—small component elements of responses, not of intelligence or memory as such. The success with which the cerebrum functions as a dynamic unit is its mass-function, and is the true measure of what we are pleased to call intelligence.

Is the mechanism of mental life such that it can be explained from the conditioned reflex standpoint? From the side of the psychologist the answer is affirmative. Agreement is not unanimous, but the wide acceptance of the concept as a fundamental mechanism is impressive. From the physiologist's viewpoint it is possible to develop an hypothesis which will include all the known facts, the best example being Forbes' (5) analysis of modern experimental findings on nerve conduction. He includes the researches of Lucas, Lillie, Nernst, Gasser, Sherrington, Lapicque, Adrian and others, as to the nature of nerve current and the limitations imposed by it upon theoretical interpretations of the functions of the central nervous system. In answer to the general question, can cortical function be explained in terms of the principles governing spinal reflexes? Forbes concludes that it can, and proceeds to outline a tentative solution of the problem. He makes no claim to absolute correctness, but points out the limits within which speculation must be bounded, and shows that the principles of objectivity and parsimony are on his side.

Aside from the strictly cortical problems involved, two researches have gone into the rôle of the receptors in learning in a way that demands consideration here. Lang and Olmsted (11) studied the motor reflex in the dog. One conditioned reflex was established to pain, with a buzzer for stimulus,

and another to pressure, with a bell for stimulus. This was on the left rear paw. Later the cord was hemisected on the right side, in the lumbar region, and the pain reflex disappeared, but not the pressure reflex. Knowing the paths of afferent nerves through the cord, they knew that they had cut the pain nerve and left the pressure nerve untouched. This seems to indicate that the primary receptor still has a definite rôle in the conditioned response, and has led one prominent text⁷ to ask, Is the conditioned reflex a true reflex? Aside from the fact that the traditional idea of the nerve pathways involved was incorrect, no new facts are brought out by this study. It is still evident that the reflex requires a specific pathway for its functioning, and its latent time (human subjects) is less than that of a voluntary reaction.

In addition, mention must be made of Lashley and Ball (14) on the rôle of kinesthetic sensitivity in the maze habit. Since sensory controls other than visual were not employed, and not all of the afferent tracts were cut in the operations employed, they have not definitely eliminated the use of kinesthesia in maze running, nor have they shown that, after acquiring a habit, an animal can use it without the sensory modality employed in learning.

"Is all learning, then, simply 'conditioning'?"⁸ Not in the sense of being passive and externally determined, as was the case with Pavlov's experiments. Motives and other internal conditions, stimulation and the external relations of the stimulus object to the organism, influence and facilitate human learning. But in the sense that all learned responses depend upon the conditioning of reflex (innate) responses, involving the mechanisms of substitute stimulation, then all learning is 'conditioning,' or at least bears an uncommonly close resemblance to it. From the psychological point of view the correspondence of conditioning to learning is apparently fundamental.

From the neurologist's point of view the conclusion is not so clear, but I believe that the balance of probability favors

⁷ F. A. C. Perrin & D. B. Klein, *Psychology*, H. Holt & Co., 1926, p. 104.

⁸ R. S. Woodworth, *Psychology*, H. Holt & Co., 1929, p. 175.

the specificity theories. My reason for this is chiefly that *learning which leaves no physiological trace in the neural mechanism does not seem to have any meaning*, and if such a trace does exist, then it must have a location. I cannot conceive of a disembodied nervous pattern any more than I can of a disembodied mind.

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CHANCE AND THE CURVE OF FORGETTING¹

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That forms evolve is generally accepted to be a fact, but as to the characteristics of the process, there are many divergent opinions. The origin of life, the possibility of causes characteristic of organisms, the operation of the law of probability, natural selection, and many other questions are raised which offer difficulty in solution. When satisfactory solutions are achieved they will be based on data from many sources.

That organisms evolved from inorganic materials is a widely accepted belief, but the manner of this change is mildly debated. On the one hand are those who dismiss the issue on the assumption that this most difficult problem of evolution may be explained by the operation of chance. On the other hand are those who are attempting to demonstrate an orderly change from the inorganic through the organic materials to organisms, in the light of the operation of the natural laws and present stellar conditions.

As the horizon of science recedes, we progress from the uncertain to the more probable and we cease to rely on accident for the explanation of phenomena and demonstrate the action of the natural laws. If there were no limiting conditions, there would exist the probability that quite by chance inorganic materials in just the correct quantities and compounds might congregate at one place under just the correct conditions to produce life. But it must be borne in mind that probability is to be invoked only when there are no limiting conditions and that if limiting conditions may be demonstrated the law of probability must be rejected. For example: there exists, mathematically, a relatively large probability that all the

¹ The author is indebted to Professor F. H. Pike and Dr. W. S. McCulloch for many valuable suggestions.

particles which make up this sheet may start moving in one direction at one time. If we multiple this probability by the product of all the objects and all the observers in the universe, we should arrive at an astonishingly large figure which would represent the frequency with which such events occur. But since no one has ever observed such a phenomenon, it becomes increasingly probable that there are some limiting conditions to the behavior of the particles which go to make up physical objects.

In the field of psychology there are some observations which are regarded either as queer coincidences or with suspicion, which upon examination prove to be by necessity rather than by chance. If man evolved subject to the natural laws, certain general conditions must follow which will be closely related to inorganic phenomena.

A case in point is that of learning and forgetting. The work in this field was first attempted by Ebbinghaus and though the mass of verbiage that has followed his results has largely obscured the essence of his findings, there is no doubt that Ebbinghaus, with all his crude methods, found the heart of the problem. Expressed mathematically, forgetting is a logarithmic function. That it is logarithmic is usually regarded as due to chance, if not to the efforts of the investigator, and many ingenious methods have been used to demonstrate the latter. If we take a broader view of the problem it becomes evident that chance played but a very minor rôle, if any at all, in determining the form of the curve of forgetting. The logarithmic form is not a chance but a necessity. If the curve took any other form we should be forced to fall back on some form of vitalism and give up our belief in evolution.

The curve of forgetting bears a marked resemblance to a group frequently met by the physicist. In any system which is out of equilibrium, that is, one in which energy is stored, the energy will be dissipated in a definite way as soon as the conditions permit dissipation. For example, the loss of a charge from a condenser, the loss of heat from a radiator, the decrease of electrical energy on a line when a circuit is

opened, the loss of head from a water tank, the dissipation of the energy of an explosion, the decomposition of a dead organism; all these and every other change of liberated energy, progress as a logarithmic function. Every chemical reaction of whatever order of magnitude follows the same function. In neither the physical nor the chemical system does it matter whither the sink is finite or infinite since the influence of this factor is only to vary the order of magnitude of the change and not the function. In order to put a system out of equilibrium work must be done on it. If the system loses energy while it is gaining it, it is obvious that no great amount of storage will take place unless energy is delivered at a rate greater than that of dissipation.

Let us now consider learning. When we learn a list of nonsense syllables work is done, not on the list but on the organism or some system within the organism. If the work is done rapidly enough the equilibrium of the system will be lost as will be indicated by the acquisition of a new operation. Since it is not possible to conceive of a change in function without some change in structure, a structural change must take place. This stored energy may take the form of a linear growth, an increased permeability of some membrane, an increased irritability, an increase in surface, or any one of a number of other forms.²

While it is true that an organism can react only in the direction of reducing its total energy content, it is equally true that the organism may react in such a direction as to take energy from one of its systems to build up another. When a muscle is exercised the total energy of the organism is reduced but energy is stored up in the muscle. It grows larger. In learning we have an analogous change.

Let us assume that in one repetition of a list of nonsense syllables a quantity of energy, A , is delivered to the anterior

² The relation between the curve of forgetting and some of the chemical changes have been noted by some of the biochemists. T. B. Robertson, 'Principles of Biochemistry,' pp. 532-535, is impressed by its similarity to the curve which expresses the issuance of a colloid from a colloidal into a fluid menstruum. A. P. Mathews, 'Physiological Chemistry,' p. 67, is struck with the similarity between memory and the spontaneous oxidation of linolinic acid.

end of the central nervous axis in unit time. This causes some change in structure whose functional aspect is learning which we shall call L . But while A is being delivered to the system a part of it is being lost. Forgetting is taking place. This rate of loss we will call ρ , which is expressed in percent. If we consider the delivery of energy to be continuous we may express the change as follows:

$$\frac{dL}{dt} = A - \rho L.$$

Integrating this we get

$$t + c = \frac{1}{\rho} (\log A - \rho L),$$

which may be reduced to

$$L = \frac{a}{\rho} - K e^{-\rho t},$$

in which K is a constant. This equation, however, is the one that expresses the storage of energy in any unrestrained system.

If learning is considered to be discrete rather than continuous, the above equation will not hold. In this case the equation becomes a geometric progression:

$$L = A(1 - \rho)[1 + (1 - \rho) + (1 - \rho)^2 + (1 - \rho)^3 \dots + (1 - \rho)^{t-1}].$$

This may be reduced to

$$L = A(1 - \rho) \left[\frac{1 + (1 - \rho)^t}{1 - (1 - \rho)} \right]$$

which again is logarithmic.

If we turn our attention to the process of forgetting it is obvious that here we are dealing with a continuous process and in which the important factors are L , the concentration of energy in the anterior end of the central nervous axis, p , the rate of dissipation of this energy, and t , the time elapsing. Forgetting we may then express as follows:

$$F = L(1 - \rho^t).$$

From the above it naturally follows that the 'physiological limit' of learning is proportional to A and in any given learning project it will be reached ³ when $L = \frac{A}{\rho}$.

If this conception ⁴ approaches a satisfactory explanation of some of the phenomena observed in the study of learning and forgetting we have one more instance in which we may dispense with the philosophy of chance in the development of organisms. Without introducing anything new in biological evolution, we have again found in the laws governing inorganic events the explanation of a phenomenon occurring in organisms.

³ One other condition may be considered in this connection. Since an organism is irritable, the energy delivered to one of its surfaces will be less than the energy delivered to the anterior end of the central nervous axis. That delivered to the receptor will be stepped up in transmission to the anterior level. This step-up we may consider to be a power factor (not analogous to the power factor of an electric circuit) and there may be some justification for assuming that it is constant. The relation between the energy delivered to the end-organ and that delivered to the more anterior level may then be expressed as: $W \cdot F = W'$ in which W is work done on the receptor, F is the power factor, and W' is work done on the anterior end. If F is high for any system, less work will be done on the receptor to produce a given change on the anterior level. This will be the condition in an organism that learns readily but this alone will not determine the intelligence of the organism. The efficiency of the system will depend upon both F and ρ . Since we mean by intelligence the persisting influence of work done on the organism, or its efficiency, intelligence must be a function of the power factor and the rate of forgetting.

⁴ In the light of the present argument a fallacy may be seen in H. L. Hollingworth's differentiation between physical and redintegrative sequences. He points out that heating a poker is a physical sequence because if it is done today the poker will not show any effects of it tomorrow and it will then take just as much energy to get it back to the former temperature as it did the first time. In the redintegrative sequence, on the other hand, as in learning, the influence of the previous work will be evident on the following day. This distinction will hardly hold, since both sequences involve the storage and dissipation of energy, the only difference being in rate. If the poker were heated before it lost all its stored energy it would then follow the redintegrative sequence.

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A BEHAVIORISTIC INTERPRETATION OF CONCEPT FORMATION¹

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It is not difficult to explain how a machine can become complicated enough to respond in a variety of ways to trees and automobiles and books and other machines; but how can a machine, regardless of complication, form a *concept* or a *generalization* of something which does not exist objectively? The physicist cannot analyze a table and find 'squareness,' yet an individual can respond to tables which are conventionally known as square in a different way than to tables which are conventionally known as round. We say that he has a concept of 'squareness,' or that he can respond in the usual way to objects which we call square. Now the question is, what is squareness for the behaviorist and how does the individual learn to respond to 'square' objects since squareness does not exist objectively?

Obviously this is a difficult problem to explain from the point of view of any psychology. Thorndike has given one of the most thorough explanations of concept formation, so perhaps it would be well to first examine that. He says that "All learning is analytic," and that concepts are analyzed out of the stimulus situation.² (The learner comes in contact with a number of situations which contain the element of 'squareness' let us say.) He explains that "(1) The bond formed never leads from absolutely the entire situation or state of affairs at the moment. (2) Within any bond formed there are always minor bonds from parts of the situation to parts of the response, each of which has a certain degree of

¹ The term 'behavioristic' is now taking on such specific meaning (that of J. B. Watson's system of psychology) that I hesitate to use the term. My own conception of the problem that is treated in this paper is that it is of a biological-social origin, both terms to be defined in a purely objective manner.

² E. L. Thorndike, *Educational psychology* (briefer course), p. 153.

independence. . . . Each total situation-response bond is composed of minor bonds from parts of the situation to parts of the response."³ In other words, each $S \cdots R$ bond is a compound affair and may be represented thus: S_1 (or S_{1a} plus S_{1b} plus S_{1c} plus S_{1d} plus $\cdots S_{1n}$) $\cdots R_1$ (or R_{1a} plus R_{1b} plus R_{1c} plus R_{1d} plus $\cdots R_{1n}$). This means that when a child makes a 'gross total response' to a table, let us say, one of the minor bonds is a 'squareness' bond and may be analyzed out into what we call a 'squareness concept.' Another of these minor bonds is the color bond, another the shape bond, another weight, another height, etc., each connecting a subsidiary element of the table with a subsidiary element of the response. It now becomes evident that learning, from this point of view, consists of isolating these minor bonds from the more general 'gross total' bonds. And so Thorndike finds justification for his conclusion that "All learning is analytic."

Let us now observe just how a concept is formed. Thorndike explains that the concept 'fiveness' may be formed in at least three ways. First, have "the learner respond to the total situations containing the element in question with the attitude of piecemeal examination, and with attentiveness to one element after another, especially to so near an approximation to the element in question as he can already select for attentive examination. This attentiveness to one element after another serves to emphasize whatever appropriate minor bonds from the element in question the learner already possesses."⁴ Accordingly the learner is shown 'five boys, or five girls, or five pencils,' and asked to respond to these in *contrast with other numbers* until a bond is established between the element of 'fiveness' and the response of 'fiveness.' A second method is to have the learner respond to a series of 'fiveness' situations, each varying from the others except in the element of 'fiveness.' The "varying concomitants counteract each other leaving the field clear for whatever uninhibited bond the 'fiveness' has."⁵ "The third means

³ *Ibid.*, p. 154.

⁴ *Ibid.*, p. 159.

⁵ *Ibid.*, p. 160.

used to facilitate analysis is having the learner respond to situations which, pair by pair, present the elements in a certain context with the *opposite of the element in question* or with something at least very unlike the element."⁶ Thus 'fiveness' is contrasted with 'one-fifthness' in varying situations until the bond of 'fiveness' is formed.

On the basis of this explanation, the number of concepts that can be analyzed out of any general response is infinite. When a child first responds to a table, he responds to the 'squareness' of it although at that time he has not analyzed the squareness concept out from the 'gross total' response. The general table response also contains the concepts of shape, size, weight, density, beauty, usefulness, color, height, etc., which are only waiting to be analyzed out.

The behaviorist must disagree with Thorndike's explanation because he has no basis for postulating 'squareness,' or any other concept, into objective existence. The physicist cannot find it when he analyzes the table, nor is there any scientific evidence that objective existence contains such a phenomenon as 'squareness.' Neither can the behaviorist agree with Thorndike that the 'squareness' concept is present in any 'gross total' response to a square object, and needs only to be analyzed out in order to be learned. Suppose that a child always responds to tables by sitting on them—round ones, square ones, oblong ones, octagonal ones. The behaviorist could not agree that the child's sitting response to a square table differed from his sitting response to a round one only in the respect that a squareness concept is embedded in the former and a roundness concept is embedded in the latter. If the physicist cannot find concepts in objective existence, and if there is no evidence that they are embedded in 'gross total responses,' it would seem that we must conclude that Thorndike's explanation is hypothetical to say the least.

The behaviorist maintains that until those responses have been made which are conventionally known as 'squareness,' the concept squareness does not exist. He defines 'squareness' as the name which English-speaking people use to

⁶ *Ibid.*, p. 161.

designate that classification of responses which are made to (1) a certain group of objective stimuli with common physical characteristics (*i.e.* equal-length sides and 90-degree angles), and (2) to the concept word itself (*i.e.* the word 'squareness'). In other words, the 'squareness' concept consists of that group of responses which are conventionally made to objects of equal-length sides and 90-degree corners, and to the word 'squareness' itself. A specific example will make our explanation more clear. Johnny Brown's squareness concept consists of (1) accurately applying the word 'square' to three tables, two books, three blocks of wood and one handkerchief; (2) sorting each of the above objects out from articles of different shape; (3) verbally correcting anyone who wrongly names or wrongly sorts these articles; (4) using the word 'square' to ask for any of the above articles when they are not present; and (5) fetching any of the above articles when they are absent but designated by the word 'square.'

As Johnny learns to respond to a greater number of 'square' objects in a greater number of conventionalized ways, his concept of squareness grows and 'becomes more meaningful.' A little later it will be practically impossible to accurately define his concept because he will be able to make so many conventional 'squareness' responses to so many conventionally 'square' objects that they cannot be named. A concept cannot be defined apart from the behavior which is classified under the concept word. A group of savages may have articles of equal-length sides and 90-degree angles but if they do not have a conventionalized classification of responses characteristic to such stimuli, these articles cannot be said to be square. Articles are square only to those who respond to them in a square way. Again, let me state the behaviorist's definition of squareness—a conventionalized classification of responses to a conventionalized classification of stimuli designated by the language symbol 'square.' Dr. Weiss explains a concept (he uses the word 'generalization') as follows:

"Because the word response is independent of the sensory nature of the stimulus, many different stimuli may release the same word

reaction. This form of behavior is known as generalization, and the process may be described as the generalizing function of language. As a behavior category generalization is a type of sensorimotor mechanism in which many different receptor patterns representative of many different sensory situations and relations, are connected to the *same* language response and through this common path the individual may react in a specific manner to all the objects situations, and relations thus connected, even though there is very little sensory similarity between them."⁷

Now the question arises—how can concepts be formed by man if he is only a machine which operates in accordance with the laws of lever movement? Can typewriters or automobiles or cotton gins form concepts, or is concept formation a non-mechanical behavior which emerges only on the human level? The behaviorist's answer is that it is possible only by those machines which are complicated enough to make language responses. A less complicated machine can be conditioned to substitute one stimulus for another (which is a rudimentary form of language), as when the ringing of a bell stimulates the conditioned dog to respond as he would to a piece of meat, but it is only a highly specialized language-using machine that can substitute one stimulus (a word) for a group of other stimuli, as the word 'food' becomes a substitute stimulus for bread and meat and potatoes and a hundred other eatables. Let us take a specific example of one way in which a concept may be formed.

The child already has a set of responses which he makes to apples—such as peeling, eating, hiding, cooking; another set of responses which he makes to bread—toasting, soaking in milk, spreading with butter; another set of responses which he makes to meat—boiling, feeding it to the dog, going to the market for it; and another set of responses which he makes to milk—drinking, pouring into a glass, and feeding to the cat. As the child grows older he learns more responses to make to these eatables, among which is the verbal response 'food.' To an apple he makes the responses peeling, eating, hiding, cooking, throwing, picking off a tree, and *saying the word 'food'*; to the bread stimulus he makes the responses toasting,

⁷ A. P. Weiss, *A theoretical basis of human behavior*, p. 297.

soaking in milk, spreading with butter, breaking into pieces, feeding to the cat, and *saying the word 'food.'* Up to this stage the word 'food' is only an additional way of responding to that class of objects which can be eaten. But the nurse is anxious to teach the child to talk, so when she brings eatables into the room (they now become visual stimuli and set off all sorts of eatable responses), she keeps them beyond reach of the child until he makes the verbal response '*food.*' She wants him to learn that the word-response 'food' is a conventionalized symbol for that group or class of objects which can be eaten.⁸ Thus when he sees eatables which are beyond his reach, he learns that if he makes the verbal response 'food,' the nurse will bring them to him.⁹ So he learns to utter the verbal sound 'food' in order to get eatables, just as he learns to make an arm and hand reaching response in order to pick up an orange.

The word 'food,' in addition to being a response to eatables, is a *means* of getting eatables. It becomes a *tool* for bringing a class of stimuli within reach of the child. The biophysical response 'food' has now, by conditioning, assumed a 'biosocial' significance. It is not only a response to eatables but it is also a stimulus which can be used to bring about changes in the objective situation.

The child now has two stimuli which will set off food responses—the food objects and the word 'food.' As mentioned above, the number and variety of these responses are constantly increasing. A child of five can respond to food objects in a great many more different ways than can a child of two. This is likewise true of the word 'food.' The group

* A concept is determined by social custom. Food is called 'food' by English speaking people simply because of custom. 'Abacadebra' would be just as good a word. Furthermore, the classification is purely a social custom. The word 'food' could apply only to those eatables which are served in hospitals, or on board ship, or in lumber camps. After all, concept formation is largely a task of learning social customs, or the biosocial significance of certain biophysical responses.

* The word response 'food' is not necessarily set off only by the *sight* of food. Hunger responses may act as stimuli to set off any of the eating responses, one of which is the word 'food.' If so, and if the nurse immediately provides eatables, she will aid the child in establishing an association between the word 'food' and the eatable articles. He learns to say 'food' in order to get eatables.

of ways that the child, of either age, can respond to the word 'food' is not identical with the group of ways in which he can respond to food objects. For example, to the word 'food,' he can write it, speak it, send it through the mail, etc., while to food objects, he can eat them, feed them to the cat, buy them at the store, etc. There are some responses which the child can make to the word 'food' which he can also make to food objects, but there are other responses which he can make only to the word 'food,' and still others which he can make only to the food objects. The concept *food* consists of those responses which are made to (a) food objects, and to (b) the word 'food.'¹⁰

The physiological processes which take place in the formation of concepts, or generalizations, are so far unknown and the behaviorist is not willing to conjecture as to what they might be. However, he does make this negative assumption—whatever those physiological processes may be, they do not violate the laws of mechanical movement. He maintains that concept formation, like all other human behavior, is mechanical movement which could be demonstrated were it possible to observe all the details of bodily movement. To the argument that bodily movement, granted to be mechanical, is only *the cause* of the concept, which is non-physical and non-mechanical in character—the behaviorist has no answer. It has never been possible to prove that witches do not exist, nor is it possible to prove that concepts are not teleological in character.

In summarizing the behavioristic account of concept formation (generalization), Dr. Weiss says,

"The generalization function of language organizes the whole repertory of reactions which the individual possesses into groups and subgroups which are made available through appropriate language stimuli, without the need of the stimuli from the actual objects or situations. This makes possible an almost unlimited

¹⁰ It should be noted here that when a concept is being formed, it is accompanied by the formation of a 'not-concept.' That is, when the concept of food is being formed, the concept of not-food is also in the process of development. The child learns both what is food and what is not food. This simply means that he applies the word 'food' to a certain group of stimuli, and the words 'not food' to everything else.

refinement of behavior categories. Such relations and discriminations between objects that are expressed by such terms as acceleration, pitch, irrational manner, atomic heat, justice, science, would be impossible without this generalization function. Generalization from the biosocial standpoint only means that an artificial set of stimuli (words) may release responses that are entirely dissimilar from the original sensory learning conditions."¹¹

Thus, while a concept for Thorndike is embedded in a general response and can be learned only by analyzing it out of that response, for the behaviorist it is a group of responses which have been classified together and labeled by a verbal symbol. Concept formation is the process of making responses of a certain type and then labeling them according to social custom. A specific concept can be defined only by naming all the responses and stimuli which are conventionally classified under the concept word.¹²

¹¹ A. P. Weiss, *op. cit.*, p. 299.

¹² The question as to how a particular concept was developed as a special conventionalized response is to be answered only by an analysis of how the discrimination arose in some individual and how it has been modified by other individuals until it has acquired its present dictionary meaning. This process may be either an historical analysis or a study of the genetic development of some specific concept in some child or group of children. There is no concept-forming faculty in the mind or brain.

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FREUDIAN INFLUENCE ON ACADEMIC PSYCHOLOGY

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In this study of Freudian influence upon American academic psychology, all the more widely used texts from 1910-1930 have been examined. The purpose of the investigation has been to ascertain, if possible, something of the status of Freudianism in academic psychology today. The accompanying tables show the results arranged chronologically in five-year periods. The analysis has been both qualitative and quantitative, thus constituting a rather thorough canvas of the field.

Procedure.—The fifty books considered in this study are all textbooks of general psychology intended for classroom use. Texts in educational, social, applied, abnormal psychology as well as 'popular' psychologies have been excluded. Revisions by the same author have been considered in order to discover changes in attitude and value attached to Freudianism at various periods. Each book has been carefully investigated to ascertain the number of pages directly devoted to a discussion of Freudianism and also the number of pages which show a marked indirect influence of Freud. In order to make a fair basis for comparative judgment, these data have in each case been compared with the total number of pages in the book. The same procedure was followed for the specific subject of *dreams*, which was thought to be a typically Freudian topic, and hence might constitute a fair indicator of the strength of Freudian influence. The particular Freudian topics treated in each text were also recorded in a table. All the data were arranged in chronological order so that the advance or decline of Freudianism might be traced with some degree of accuracy.

TABLE I

Author	Date	<i>T</i>	% <i>F</i>	% <i>Tf</i>	<i>Td</i>	% <i>Fd</i>	% <i>TFd</i>	<i>A</i>
Calkins.....	1910	399	.06	.06	3	8.33	8.33	±
Titchener (<i>T</i>).....	1910	552	.00	.00	0	.00	.00	0
Yerkes.....	1911	416	.00	.00	0.5	.00	.00	0
Dunlap (<i>S</i>).....	1912	354	.00	1.60	0	.00	.00	—
Phillips.....	1913	342	1.70	3.20	2	100.00	100.00	++
Calkins.....	1914	400	.06	.06	3	8.33	8.33	±
Münsterberg.....	1914	470	.00	2.90	3	.00	100.00	±
R. M. Ogden.....	1914	264	.70	2.60	3	33.00	33.00	—
Titchener (<i>B</i>).....	1915	349	.00	2.29	7	.00	28.57	/
Pillsbury (<i>F</i>).....	1916	554	.18	.18	0	.00	.00	±
Breese.....	1917	462	.43	1.29	4	50.00	75.00	±
Judd.....	1917	348	.00	.00	1.5	.00	.00	±
Angell.....	1918	272	.18	.52	1	50.00	100.00	±
Warren (<i>H</i>).....	1919	449	.10	2.60	5	20.00	20.00	—
Watson.....	1919	420	.00	7.80	1	.00	100.00	±
Pillsbury (<i>E</i>).....	1920	422	.70	1.42	2	100.00	100.00	±
Smith and Guthrie.....	1921	260	.76	5.00	2	.00	100.00	/
Woodworth.....	1921	570	1.75	4.03	16	43.70	100.00	±
Dunlap (<i>E</i>).....	1922	359	.00	.00	0	.00	.00	0
Givler.....	1922	373	.50	7.05	11	.00	100.00	+
Pillsbury (<i>F</i>).....	1922	582	2.70	4.12	5	100.00	100.00	—
Warren (<i>E</i>).....	1922	382	.70	4.18	5	.00	.00	—
Griffith.....	1923	497	2.40	6.60	12	25.00	41.60	+
Hunter (<i>G</i>).....	1923	358	1.39	2.00	0.25	100.00	100.00	+
McDougall.....	1923	450	.60	1.30	0	.00	.00	—
Seashore.....	1923	408	.20	6.10	23	2.17	30.40	+
Bentley.....	1924	532	.50	1.00	0	.00	.00	/
Kantor.....	1924	462	.10	.97	1.25	.00	.00	±
Watson.....	1924	440	.00	7.70	1	.00	100.00	±
Carr.....	1925	424	.00	.94	0	.00	.00	0
Gates.....	1925	580	1.30	8.96	4	.00	25.00	—
Gault and Howard.....	1925	460	.00	2.60	6	.00	83.30	±
Cole.....	1926	350	.50	1.14	4	25.00	50.00	±
C. K. Ogden.....	1926	312	8.00	16.60	7.5	4.00	100.00	++
Perrin and Klein.....	1926	373	3.20	8.80	1	100.00	100.00	+
Pyne.....	1926	369	.00	1.60	4	.00	50.00	—
Robinson.....	1926	471	.00	4.20	17	.00	100.00	±
Troland.....	1926	248	1.60	7.60	0	.00	.00	—
Lund.....	1927	482	.62	2.70	12	16.60	66.66	±
Thomson.....	1927	473	4.86	17.00	11	27.20	100.00	+
Dashiell.....	1928	571	.17	1.40	0	.00	.00	+
Gates.....	1928	596	1.30	8.22	4	.00	25.00	—
Griffith.....	1928	591	3.55	8.29	16	18.70	37.50	++
Hollingworth.....	1928	487	.00	5.13	6	.00	33.33	?
Hunter (<i>H</i>).....	1928	343	1.40	3.50	0.25	100.00	100.00	+
Ruckmick.....	1928	232	8.00	2.50	1	.00	.00	/
Rexroad.....	1929	375	.00	1.00	0	.00	.00	±
Wheeler.....	1929	525	1.50	6.00	4	75.00	75.00	++
Woodworth.....	1929	580	1.37	5.80	14	42.80	100.00	±
Pillsbury (<i>E</i>).....	1930	459	.65	1.52	2	100.00	100.00	±
Warren and Carmichael (<i>E</i>).....	1930	390	.19	1.98	7	7.00	14.28	±

Abbreviations by author's name—*T* = Textbook; *S* = System; *B* = Beginners; *F* = Fundamentals; *H* = Human; *E* = Essentials or Elements; *G* = General.

In this table the pages enumerated as containing explicit Freudian material represent the total actual pages devoted to a treatment of Freud. Thus, a footnote referring to Freud, or a mere mention of the Freudian movement has been counted as only a small fraction of a page. A mere bibliography reference has not been counted as direct evidence. The total pages of Freudian influence include both direct treatment and indirect influence shown by Freudian attitude toward various subjects, by treatment of typical Freudian topics, and by use of Freudian phraseology such as repression, symbolism, complex, libido, regression, bipolarity, fixation, rationalization, psychoanalysis, wish fulfillment, sublimation, compulsions, defense mechanisms, etc.

The total pages in the text were counted exclusive of appendices, indices, bibliography, and review questions at the end of the book, with the exception of Miss Calkins' books in which the appendices comprise almost one third of the material. In this case it was thought only fair to count the appendices in the total number of pages.

A few authors, Titchener, Dunlap, McDougall, Carr, Troland, Dashiell, and Rexroad, give no space at all to the subject of dreams. In most cases this may be explained on the basis of the author's particular psychological bias, that of Existentialism, Functionalism, Behaviorism, etc., or on the basis of his purpose in writing the book. It might be mentioned that the total material devoted to dreams is counted as including the subject of day-dreams. The percent of Freudian treatment of dreams and of total Freudian influence

Key to Table I

T = total number of pages in the text.

$\%F$ = material directly treating Freudianism, compared with T .

$\%Tf$ = total Freudian influence, direct and indirect, compared with T .

Td = total material devoted to dreams.

$\%Fd$ = direct Freudian treatment of dreams compared with Td .

$\%TFd$ = total Freudian influence, direct and indirect, on dreams, compared with Td .

A = attitude of the author toward Freudianism, as evidenced by his book.

(Symbols: ++ strongly Freudian; + moderately in sympathy with Freudianism; - strongly anti-Freudian; / moderately anti-Freudian; \pm unbiased attitude, sometimes rather indifferent, sometimes presenting both sides in an unprejudiced manner; o no indication; ? doubtful.)

is found by a comparison with the total material devoted to dreams.

Table II is a qualitative analysis showing the various Freudian topics discussed in each book which contains a

TABLE II

Author	H	J	E	L	P	F	R	S	D	U	C
Phillips.....					*				*	*	
R. M. Ogden.....					*		*	*	*		*
Breese.....		*			*		*		*	*	
Angell.....					*		*		*	*	
Warren.....			*		*		*	*	*	*	*
Smith and Guthrie.....						*	*		*	*	
Givler.....				*			*		*	*	
McDougall.....		*					*	*	*	*	*
Seashore.....					*		*		*	*	
Bentley.....		*			*		*	*	*	*	
Kantor.....		*			*		*	*	*	*	
Watson.....			*	*	*		*	*	*	*	
Cole.....					*		*	*	*	*	
C. K. Ogden.....		*			*	*	*	*	*	*	
Perrin and Klein.....		*			*		*	*	*	*	
Pyne.....							*	*	*	*	
Troland.....							*	*	*	*	*
Lund.....							*	*	*	*	*
Thomson.....		*	*		*		*	*	*	*	*
Dashiell.....					*		*	*	*	*	*
Gates.....			*		*	*	*	*	*	*	*
Griffith.....	*		*	*	*	*	*	*	*	*	*
Hunter.....			*	*	*	*	*	*	*	*	*
Ruckmick.....			*		*		*	*	*	*	*
Rexroad.....					*		*	*	*	*	*
Wheeler.....	*	*			*	*	*	*	*	*	*
Woodworth.....		*	*		*	*	*	*	*	*	*
Pillsbury (F).....		*	*	*	*	*	*	*	*	*	*

Key to Table II

- H* = history of the psychoanalytic movement or of Freud's life.
J = references other than bibliography to Jung, Adler, and others of Freud's followers.
E = the psychology of error, slips of the tongue and pen, etc.
L = Freud's theory of wit and laughter.
P = treatment of the method or of the school of psychoanalysis.
F = Freud's theory of forgetting.
R = repression, complexes, conflict, mechanisms, etc.
S = the *libido* or other reference to the sexual drive.
D = the psychology of dreams and their interpretation, symbolism.
U = the unconscious and the doctrine of the censor.
C = criticism of Freud's theories.

direct treatment of Freud. In this table, it was thought unnecessary to list the various revisions of the same book, as practically no differences in type of material were found. Hence in each case, the latest edition was used. One name (Watson) was included in the list because of the large amount of Freudian influence shown, although Freud is mentioned only in the bibliography. The chronological order and the divisions into five-year periods were followed as in Table I.

The topic headings of Table II were chosen not because they necessarily are typical Freudian subjects, but because of the frequency and manner of their treatment in the various texts. Hence, some relatively unimportant subjects such as the theories of laughter and forgetting are listed as separate headings, while repression, complexes, rationalization, defense mechanisms, etc., are grouped as one. The asterisks indicate the type of material treated by each author. This treatment of Freudian topics does not necessarily indicate that a given author accepts the Freudian doctrine. It only indicates that the author recognizes Freudian theories and thinks them worth considering in his textbook.

RESULTS: AMOUNT OF FREUDIAN MATERIAL

Of the fifty textbooks investigated, four, or less than eight percent, ignore Freudianism entirely both directly and indirectly. These, Titchener (1910), Yerkes (1911), Judd (1917), Dunlap (*E*) (1922), may be accounted for on the basis of the early date or on the basis of a strictly objective viewpoint, or again, perhaps because of the personal bias of the author. Several others (15) do not care to give space in their texts to a direct discussion of Freudianism, but cannot escape the indirect influence. With the exception of Dunlap, whose anti-Freudian tenets are well known, we find no text since 1917 which fails to show the influence of Freudianism.

Parenthetically, I do not fail to realize the vagueness of the term, *influence*, nor the errors of judgment which beset the way of the investigator of trends and attitudes. I have tried to be conservative in my estimates and have not counted pages devoted to discussion of drives, hypnosis, split person-

ality, etc. as Freudian influence unless the treatment seemed to have the Freudian twist, that is, unless it seemed in accord with Freudian notions, or at least recognized Freudian ideas and used a partially Freudian vocabulary. The material counted satisfies these conditions as nearly as I can judge.

It will be noticed that the most attention to Freudianism seems to center around the years 1924-1928. Watson's book in 1924 shows 7.70 percent, while C. K. Ogden's in 1926 shows 16 percent, and Thomson's of the next year, 17 percent of material which is dominated by Freudian theory. The two editions of Gates 1925, 1928 each have over 8 percent of material showing Freudian influence. This is also true of Perrin and Klein (1926) and Griffith (1928). Wheeler and Woodworth in 1929 with 6 percent and 5.1 percent respectively seem to indicate that Freudianism is still an influence in academic psychology. The average percent of Freudian influence (where shown) during this period from 1910-1930 is 4.07; not large, to be sure, but significant if compared with the amount of material expressly influenced by the theories of any other one man.

The distribution of pages as well as the mere quantity of influence indicates something not discernible in the tables. For instance, Ogden and Thomson's texts are permeated with Freudian influence throughout, and Wheeler's thirty-two pages showing Freudian dominance have a distribution of thirty-eight pages ranging from page 23 to page 345, with not more than twelve pages in succession and with several references of a few lines each. Such instances show a spreading of Freudian influence which is difficult to measure and which is often underestimated.

At the beginning of this study it was thought that the trend of sentiment toward Freudianism in academic psychology might be determined somewhat from a perusal of the various editions and revisions over a period of years of texts by the same author. The results are indeed rather illuminating.

Miss Calkins' revisions of 1910 and 1914 show no differences in the very slight references to Freud. This is probably

to be expected in her case, because of her particular view of psychology. Titchener's texts in 1910 and 1915, being really two different books, the 'Textbook' and the 'Beginner's Psychology' respectively, probably have little significance in this regard, although the later text shows over two percent general influence and over twenty-eight percent Freudian influence on the subject of dreams, in contrast with no influence evident at all in the earlier book. The different purposes of the two texts probably account for this variation. The same may be said of Dunlap's two books, 1912 and 1922. A decidedly negative attitude is evidenced indirectly in the earlier book, the 'System of Psychology,' while Freud is entirely ignored in the 'Elements of Scientific Psychology.' This, as in the case of Titchener, is undoubtedly due to the author's purpose in writing the book rather than to any change in attitude toward Freudian doctrines.

Pillsbury's four books should perhaps be indicative of something. But again we have two different texts and consequently two different viewpoints to consider. Between the 'Fundamentals' of 1916 and 1922 there is a decided increase in the amount of Freudian material stressed, over three percent actual increase in general influence (almost 23 times the amount in 1916), and an inclusion in the 1922 edition of the subject of dreams which is given a decidedly Freudian treatment. This seems to be significant, although the author's attitude seems to be quite unfavorable toward Freudian theory. The 'Essentials' of 1930 shows a slight increase of attention to Freud, *i.e.* one tenth of one percent. The pages devoted to dreams show a dominant Freudian influence in both editions, 1920 and 1930. Pillsbury's attitude toward Freudian theory seems to be rather neutral in both editions of the 'Essentials.' He rejects and criticizes certain views which he considers extreme but sees value in others.

In Warren's two books, we find about one and two thirds times as much space given to Freud in the 1922 edition as in the 1919 edition, this in spite of the omission of a section on dreams in the later edition and in spite of the author's negative attitude.

The figures in the case of Watson's books, 1919 and 1924, are rather misleading. Because of an increase in the total number of pages in the later edition the percent of Freudian material is slightly less in that edition. In actual number of pages, there is a slight increase of Freudian influence.

Woodworth's 1929 edition shows a twenty percent proportional increase in Freudian influence over the 1921 edition. The treatment of dreams remains distinctly Freudian and the author's attitude is consistently one of impartial criticism, accepting certain parts of Freudian theory and rejecting others.

Between Griffith's editions of 1923 and 1928 there is a proportional increase of almost five percent of Freudian material despite the fact of a slightly more than three percent proportional decrease in the amount of Freudian emphasis on dreams. The author's attitude seems even more favorable toward Freudianism.

Hunter's books of 1923 and 1928 are different texts, but we find a proportional increase of over 50 percent of Freudian material in the 1928 publication. The section on dreams remains dominantly Freudian and the attitude of the author favorable.

The decrease in the amount of Freudian influence seemingly shown in Gates 1928 edition is due to the fact that the later edition contains several more total pages than the 1925 text. Actually, there is in the 1928 edition, about a page and a half additional which shows Freudian influence. The author's negative attitude is evident in both editions.

These investigations indicate that in every case where an author has published a second edition of the same text, the later edition has shown no actual decrease in the amount of space influenced by Freudian theories, but with one exception (Calkins), an actual increase, and in most cases a quite pronounced proportional increase. This increase is the more significant because it holds true of authors whose attitude toward Freudianism is unfavorable as well as those who are distinctly of the Freudian persuasion. From these figures, it would seem that Freudianism is steadily gaining an important place for itself in academic psychology.

Of the authors who take up a study of dreams (42), eighteen show 100 percent Freudian treatment either explicit or implicit. On the other hand, six texts which treat of dreams show no Freudian influence. Of these, however, only one has more than one page devoted to the subject, hence the purpose of the author would seem to have precluded any extensive discussion of dreams. Warren devoted five pages to the treatment of dreams in his 'Elements of Human Psychology' (1922) with no mention of Freud, while in his earlier text (1919) Freud was treated. This seems to indicate a change of emphasis only, because the later book shows a general increase of attention to Freud of one and one half percent over the previous edition. We may surely be justified in concluding from these data that the prevalent attitude toward dreams takes account of Freud and his theories.

TYPE OF FREUDIAN MATERIAL

As might be expected, the texts which show the most Freudian influence also cover a wide range of Freudian topics. The subjects most universally treated seem to be those of repression and complexes. Only three of the texts listed here omit these topics. The topics next in popularity are the Freudian treatment of the unconscious, of dreams, of sex or the 'libido,' and of the psychoanalytic movement. The type of Freudian material treated seems to vary but slightly over the period of twenty years.

It might be interesting to note the prevailing sentiment toward various Freudian doctrines. In general, most of the texts which contain any criticism of Freud agree in rejecting the elaborate mechanism of the unconscious, with its guarding censor. Most of the texts also agree that Freud has over-emphasized the place of the 'libido' or the sexual drive. This criticism, in many cases, is not of the essential Freudian theory but only of its extreme applications.

In general, those who give any attention to Freud at all, accept his theory of dreams as wish fulfillments as at least a partial explanation. They also accept his theory of free association in psychoanalysis as valuable in locating and cor-

recting complexes, phobias, and compulsions of various kinds brought on by repressions of some sort. Many who disagree with the Freudian doctrine in its details, nevertheless admit its influence and value as a new psychological movement.

OPINIONS

A few statements from representative authors both for and against the Freudian movement may show prevailing attitudes more specifically than can be indicated in the table.

Wheeler mentions three outstanding events in psychology during 'the last fifteen years.' The first of these was a reaction against the traditional psychology of consciousness, a reaction which in America took the form of Behaviorism, in Germany of Gestalt psychology. The development of social psychology was the third important event. In Wheeler's own words, "Freud's concerted attack upon problems of the emotional life of human beings was the *second* important event of recent years. Prior to Freud, investigations of feeling and emotion had resulted in little if any progress because the individual was being studied in artificial, laboratory situations. He and his followers pointed the way to a much better understanding of human nature by analyzing the entire emotional situation including its social conditions."¹

Troland voices a negative attitude. With the advent of the Freudian psychology, "We are now at liberty to glory in the reduction of benevolent intentions to sexual and egotistical wishes."² "The Freudian literature tends to emphasize and to overemphasize the kinds of complexes which we regard as abnormal and reprehensible."³

McDougall calls Freudianism 'the latest and most fashionable fad in psychology.'⁴

However, this fad cannot be ignored, says Thomson.⁵ "All, however, will agree that Freud has made valuable

¹ R. H. Wheeler, *The science of psychology*, New York, Crowell, 1929, pp. 23-24.

² L. T. Troland, *The mystery of mind*, New York, Van Nostrand, 1926, p. 9.

³ L. T. Troland, *op. cit.*, p. 156.

⁴ Wm. McDougall, *Outline of psychology*, Chicago, Chas. Scribners Sons, 1923, p. 126.

⁵ M. K. Thomson, *The springs of human action*, New York, D. Appleton and Co., 1927, p. 11.

contributions. The alleged discovery of a new source of motivation in the subconscious in the form of repressed desires is significant for our problem."⁶

'Interesting, but not entirely convincing' says R. M. Ogden⁷ of the Freudian theory of dreams. "It is doubtful whether all hysteria is attributable to sexual experience, especially the experience of childhood, as Freud maintains."⁸

Gates⁹ says, "When explained in detail, the Freudian concepts are fascinating and often convincing. . . . The trouble is that while they fit in well with popular notions, the concepts are really scientifically unsound."

While being quite strongly influenced by Freud, at the same time, Woodworth sees clearly many flaws in the theory. "Freud has given an 'impressionistic' picture [of human motives],¹⁰ very stimulating and provocative of further exploration, but by no means to be accepted as a true and complete map of the region."¹¹

Griffith thinks that "it is a misfortune for psychology that so much of Freudianism is a veritable riot of the imagination. In spite of the fanciful part, however, there is a profound truth and a profound challenge in its program."¹²

Pillsbury admits that the Freudian hypothesis "has proved of practical value in suggesting means of treating patients,"¹³ but he contends that the "explanation is so general that it applies to everything and therefore to nothing."¹⁴

From these statements we may conclude that while the Freudian theory is severely criticized, yet it continues to demand recognition and will not be brushed aside as irrelevant. Even its opponents admit its partial truth and its essential

⁶ *Ibid.*, p. 289.

⁷ R. M. Ogden, *An introduction to general psychology*, New York, Longmans, Green and Co., 1914, p. 221.

⁸ *Ibid.*, p. 240.

⁹ A. I. Gates, *Elementary psychology*, New York, Macmillan Co., 1928, p. 267.

¹⁰ The bracketed phrase is my insertion. The meaning is Woodworth's.

¹¹ R. S. Woodworth, *Psychology*, New York, Henry Holt Co., 1929, p. 486.

¹² C. R. Griffith, *General introduction to psychology*, New York, Macmillan Co., 1928, p. 391.

¹³ W. B. Pillsbury, *The fundamentals of psychology*, New York, Macmillan Co., 1922, p. 452.

¹⁴ W. B., Pillsbury, *op. cit.* p. 451.

challenge. On the other hand, many more favorably inclined are seeing the applications of Freudianism in almost every category of psychology.

CONCLUSION

In summary, we find that of the fifty academic psychologies published between 1910 and 1930, over ninety-two percent show distinct Freudian influence. Since 1917, with one exception, every book shows some Freudian influence. Seventy percent of the texts canvassed show direct treatment of Freudian theories. Absence of Freudian influence can be accounted for on the basis of the early date of the book or upon the basis of the particular purpose or bias of the author.

The most attention to Freudianism seems to center around the years 1924-1928. Thomson's book in 1927 and C. K. Ogden's in 1926 show the highest amounts of Freudian influence *i.e.* 17 percent and 16 percent respectively. From then on till 1930, the percent seems to be fairly constant, around 6 percent, and the material seems to be more widely diffused throughout the range of topics treated in the texts. For the twenty-year period, the average percent of Freudian influence is 4.07. These figures indicate that Freudianism has a recognized place in academic psychology today and is becoming more and more firmly established.

A comparison of the revised editions of the same book shows in general, a decided increase in the amount of Freudian influence. This holds true regardless of the author's opinion of Freud, thus making the increase all the more significant.

On the subject of dreams, which was taken as an indicator, over fifty-seven percent of those treating the subject show 100 percent Freudian influence on that topic. Most other texts show a high percentage of Freudian influence. Thus the treatment of dreams is seen to be largely Freudian. The Freudian ideas of wish fulfillment, complexes, compulsions, mechanisms, repressions, and the free association method in psychoanalysis seem to be quite generally accepted as at least partial explanations.

On the other hand, there is quite a general agreement

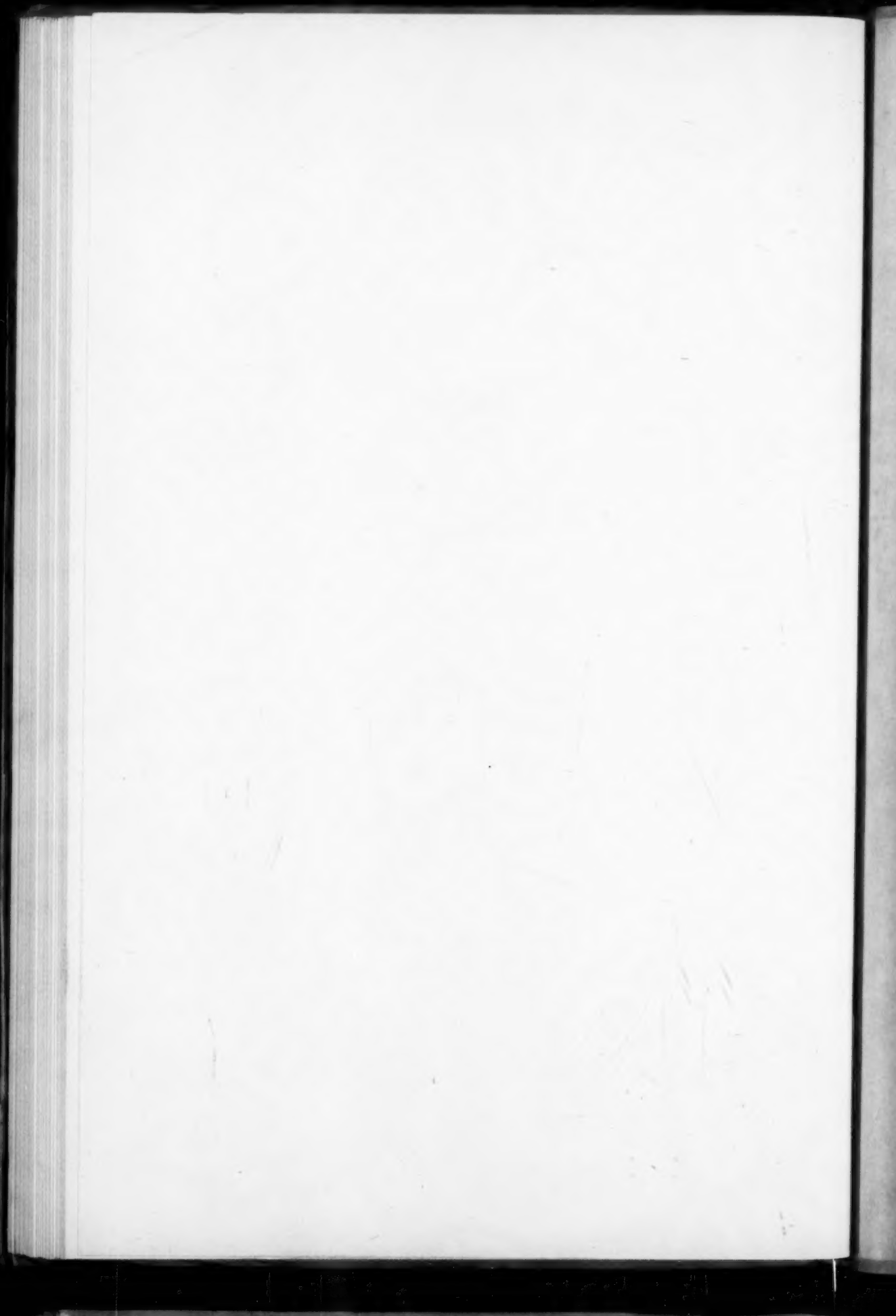
among the psychologists who have voiced criticisms, that Freud has overemphasized the place of the 'libido' or sex drive, and that he has gone to an extreme in his elaborate mechanism of the unconscious.

Freudianism has received much criticism, and perhaps justly, but even its opponents are compelled to admit that it has 'some practical value' and that it is a powerful influence in psychology today.

Our results point to the conclusion that Freudian influence has been quite steadily increasing since 1910 and is consistently holding its own at the present time despite opposition and controversy. The influence of Freudianism is evidenced in the last few years not only by an increase in the number of pages devoted to it, but by a spread of Freudian influence into most of the major categories of psychology. Thus the status of Freudianism in academic psychology seems to be rather firmly established.¹⁵

¹⁵ Since this paper was finished, a new edition of 'Elements of Human Psychology' by Warren and Carmichael has come off the press. Contrary to the results summarized in this article, the new edition shows a decrease in Freudian influence, from .7%*F* in the 1922 edition to .19%*F* in the 1930 and from 4.18%*Tf* (1922) to 1.98%*Tf* (1930). This decrease is, however, perhaps somewhat compensated for by a more liberal attitude toward the Freudian theories of dreams and of suppression.

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